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Do Temporary Demand Shocks have Long-Term Effects for Startups?*

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Abstract

Using detailed procurement auctions and register data from Norway, we find that temporary demand shocks have long-term effects on startup outcomes. Startups that win a procurement auction are more than 20% larger in terms of sales and employment than startups that narrowly lose an auction, even several years after the contract work ends. For mature firms, we do not find long-term effects of auction wins. The persistent effects of temporary demand shocks for startups appear driven by learning-by-doing effects and by winning startups undertaking irreversible investments. The results point to the importance of path dependence in shaping the long-term outcomes of startups.

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1 Introduction

Startups are important for job creation and for innovation, and given these roles there is wide interest in understanding why some startups become succesful and others do not. In this paper, we investigate the role of temporary demand shocks, or "luck", for long-term startup outcomes. The empirical opportunity comes from procurement auctions in Norway. We compare long-term sales and employment for startups that win a procurement auction with long-term sales and employment for auction runners up — that is, we compare long-term outcomes for startups that received a temporary demand shock with startups that almost did.

To motivate why temporary demand shocks could have long-term effects for startups, consider the story of Microsoft. In 1975, the year when Microsoft was founded, Paul Allen read an article about the Altair 8800 microcomputer and suggested to Bill Gates that they could program a BASIC interpreter for the device. Gates called up Altair and claimed to have a working interpreter, something they did not have at that time. Over the next few weeks, Allen and Gates worked frenetically to produce a working BASIC interpreter, achieved it, and to their surprise it worked flawlessly when they demonstrated it to Altair. This early demand shock has often been viewed as instrumental to the subsequent phenomenal growth of Microsoft.[1]

We use extraordinarily detailed data from Norway. Register data provide detailed accounting, ownership, and employment information on all Norwegian firms at the yearly level. This data allow us to identify startups, as opposed to spinoffs from established companies, through firm age and subsidiary status. The Norwegian Public Roads Administration (henceforth simply 'Public Roads'), the government agency responsible for road construction in Norway, has provided us with data on the universe of competitive procurement auctions run between 2003–2015, in total about 10,500 auctions. The typical auction involves a road-related procurement,

[1]The early Microsoft story has been told and retold many times. See, for example, https://news.harvard.edu/gazette/story/2013/09/dawn-of-a-revolution/. Microsoft revenues after five years, in 1980, were about $7.5 million, and in 1985 about $140 million, according to https://www.thocp.net/companies/microsoft/microsoft_company.htm.

but there is considerable variation in products, ranging from consulting reports to office supplies. For each auction, the Public Roads data describe the product procured, the contract work duration (typically less than a year) and, for each bid, the identity of the bidder and the bid size. The magnitude of the average bid by a startup is about 25% of sales in the prior year.

Our empirical strategy is to compare long-term outcomes for startups that win a procurement auction to long-term outcomes for runner-up startups that narrowly lose an auction. The main result of the paper is that even several years after the procurement contract work has been completed, the startups that won a procurement auction are more than 20% larger in terms of sales and employment than the startups that came second. Winning startups are also more profitable. We do not find long-term effects of auction wins for mature firms, even for the mature firms that are comparable to startups in terms of size. This suggests that the positive long-term effects of a temporary demand shocks may be unique to startups.

A main threat to identification could be differences between winner and runner-up startups not captured by a simple difference-in-difference model: it is possible that auction winners would follow a different trend than losers after the auction, even absent the auction win. That winners and runners up are balanced on observables prior to the auction alleviates some concern, and to accommodate unobserved heterogeneity, we include firm fixed effects. In addition, we perform two placebo tests. First, we compare the trends of winners and runners up prior to the auction, and show that they are the same. Second, we compare the post-auction performance of runners up with startups that end up in third place. Differences between these two groups cannot be due to winning an auction, as neither win. The startups in these two groups have very similar trends and levels on observables both before and after the auction.

Guided by theory we attempt to learn why auction winners, even years after the auction, perform better than runners up. One hypothesis is that the contract work enhances the firm's capabilities through learning-by-doing (e.g., Arrow, 1962, Thompson, 2012). We find some support for this hypothesis, in that winners expand "vertically" by becoming more likely than runners up to participate in subsequent larger auctions. Winners also expand "horizontally," by increasing their participation in auctions for new products. For example, if the initial auction involved the procurement of a bicycle path, the future auction may involve procurement of protective fence around a road. Although winning does not impact firm productivity as conventionally measured (e.g., Syverson, 2011), auction winners tend to enter subsequent auctions with higher productivity of competitors, and to hire managers of higher quality, suggestive of a latent productivity increase. In addition to learning-by-doing effects, we find evidence consistent with winning startups undertaking investments that could lead to lasting firm size differences, often referred to as 'sunk cost effects' (e.g., Sutton, 1991, Das et al., 2007). Case evidence collected from newspaper articles, presented in Section 7.3, broadly support these insights.

Our work primarily informs a large academic literature that explores factors (e.g., founder ability, access to finance, legislative framework) that seek to explain why some startups do better than others.[2] We highlight that an overlooked factor, temporary demand shocks, can play an important role for long-term startup outcomes. In contrast to much of the existing literature, our statements are causal, in that we exploit exogenous variation in temporary demand shocks at the firm-level. Two working papers, Ferraz et al. (2016) and Lee (2017) use a similar identification strategy to ours to analyze firm growth following a temporary demand shock. Neither Ferraz et al. (2016) nor Lee (2017) have information on subsidiary status, which is needed to identify startups as opposed to spinoffs, and Lee (2017) only observes the balance sheets of firms whose assets exceed $10 million, which are unlikely to be startups. In Section 5, we discuss the relation to these two papers in more detail.[3]

[2]This literature is too large to list exhaustively. A few recent examples are Farre-Mensa et al. (2019) on patent awards, Foster et al. (2016) on demand accumulation, Gompers et al (2010) on ability differences and performance persistence for serial entrepreneurs, Hall & Woodward (2010) on venture capital funding, Hvide & Jones (2018) on legislative framework, Hvide & Oyer (2018) on within-family transfers of human capital, Kerr & Nanda (2009) on financial constraints, Kerr et al. (2014) on business angels, Lerner & Malmendier (2013) on peer learning, and Levine & Rubinstein (2016) on founder characteristics.

[3]Farre-Mensa et al. (2019) find that for a sample of technology startups, an early-life patent strongly affects a startup's long-term sales and employment growth. A patent award could be interpreted as a long-term shock to firm demand, as patents protect firms from competition in a given product class for up to 20 years, while we estimate the long-run effects of a temporary demand shock, the typical procurement contract having duration less

Our work also connects to recent empirical work in the macroeconomic literature showing that firms born in cohorts with weak job creation are persistently smaller on average, even when the aggregate economy recovers (e.g., Moreira, 2016, Sedlacek & Sterk, 2017). As both demand and supply-side factors vary with the business cycle, it is hard to establish what drives these cohort effects from aggregate data alone. With the important caveat that extrapolating from micro to macro is difficult due to general equilibrium effects, our findings suggest the possibility that part of the reason for the persistent cohort effects in startup job creation documented in the macro literature can be the demand component of business cycle variations.

More broadly, our work contributes to empirical research that explores path-dependence in a variety of settings. For example, in recent work, Aghion et al. (2016) demonstrate path dependence in the type of research that R&D-intensive firms pursue. Other contexts include economic geography (e.g., Bleakley & Lin, 2012, Glaeser et al., 2015), industrial organization (e.g., David, 1985), corporate governance (Bebchuk & Roe, 1999), labor markets (Oyer, 2008, Schoar & Zuo, 2017) financial decisions (e.g., Malmendier & Nagel, 2011), and central banker decisions (Malmendier et al, 2019). To our knowledge, ours is the first paper to establish path-dependence for startups in a setting with exogenous variation in economic conditions.

The paper proceeds as follows. Section 2 provides an overview of the institutional setting. Section 3 describe the data sources and summary statistics. Section 4 describes the empirical methodology. Section 5 presents the main results on startup long-term outcomes, while Section 6 describes how startups expand. Section 7 discusses mechanisms and Section 8 concludes. The appendices contain additional theoretical and empirical analysis.

than a year.

2 Institutional background

This section provides context on Public Roads procurement auctions. In Section 2.1, we give a brief overview of Norway and its government procurement. In Section 2.2, we provide details on Public Roads and its procurement process. Appendix B provides additional institutional details.

2.1 Norway

Norway is an industrialized country with a population of approximately five million. In 2017, Norway ranked among the ten richest countries in the world, as measured by purchasing power adjusted gross domestic product (GDP) per capita, according to the World Bank (2017). Appendix A provides an overview of recent business cycles in Norway. Norway has a well-educated population with a large middle class, low income inequality and low wealth inequality. The country has a large public sector that is primarily financed through high levels of taxation and oil revenue. Norway ranks 8th out of 190 on the World Bank ease-of-doing business index. The public sector in Norway consistently ranks among the ten least-corrupt in the world, according to the Corruption Perception Index published by Transparency International.

Government procurement accounts for about 15% of GDP in Norway in 2015 (Statistics Norway, 2015). This is close to the OECD average of 12% (OECD, 2017). Norwegian government procurement is regulated by the Public Procurement Act, which aims to promote transparency and efficiency. The Act provides instructions on, among other things, whether, how, and when government procurers should announce their demands to the public; how to organize procurement auctions; and which restrictions should be imposed on suppliers in terms of minimum safety requirements, environmental impact, and fair payment to subcontractors. In Appendix B, we describe these rules and regulations in more detail.

2.2 Public Roads

Public Roads (*Statens Vegvesen* in Norwegian) is a government agency established in 1884 to build and maintain public roads all over the country. Its current assignments include, among others, the planning, construction and operation of road networks; driver training and licensing; vehicle inspection; and subsidies to car ferries. Public Roads is led by the Directorate of Public Roads, a subsidiary of the Ministry of Transport and Communications. Administratively, Public Roads is divided into five regions — Northern, Central, Western, Southern, Eastern — and 30 districts. In 2015, Public Roads had 7,585 employees and NOK 53.75 billion (USD 6.07 billion) in turnover, and accounted for about 10% of government procurement in Norway.

Figure 1 presents a timeline for how Public Roads procures products and services. First, Public Roads decides to purchase a product or service. It then chooses the auction format and contract duration, which are both announced publicly, before bids are collected and ranked. Finally, the auction winner is announced and delivery or production commences. We have been given access to exhaustive documentation on all Public Roads procurement auctions in the period 2003–2015, a total of around 10,500 unique procurement auctions.

Appendix B.2 provides a detailed breakdown of the product types that Public Roads procures using the procurement-level data described in Section 3.1. Using a broad classification, 63% of all Public Roads procurement auctions involve construction work, about 30% services, and materials the remaining 7%. Using a finer classification, Appendix B.2 shows that 19% of the auctions involve road construction work. Traffic safety measures rank second with 5% of all auctions. In other top-ten products, we find consulting services, building of fences, and construction of smaller stretches of walking and cycling paths. On auction formats, Appendix B.2 shows that 60% of the Public Roads procurement auctions are determined by price only while the rest are determined by price and additional criteria.[4] In Section 5, we focus entirely

[4]Once a procurement has been announced to the public, Public Roads must strictly adhere to the specified auction format. For example, if price has been specified as the only winning criterion, other factors such as quality, time-of-delivery, and past experiences with the supplier will not affect the ranking of suppliers. Appendix B

on price-only auctions, while in Section 6 we also use the other auctions.

3 Data

Here, we describe the various data sources, explain how the databases have been linked together, describe sample restrictions, and present summary statistics from the final sample.

3.1 Procurement-level data

We have detailed data on all Public Roads procurement auctions in the period 2003–2015.[5] In total, the data covers around 10,500 procurement auctions. For each auction, the data provide the names of all bidders and the size their bids (in Norwegian kroner), as well as the name of the winning firm. In addition, the data contain information on whether an auction was decided by price only or included other criteria, such as quality or time of delivery, and whether the procurement auction was open to all interested bidders or if competition was restricted in any way. The data also provide details on whether any of the bids were rejected on formal grounds (often because the bid missed the submission deadline).

As described in Section 2.2, the data indicate whether the procurement relates to construction work, services, or goods, as classified by Public Roads, in addition to a more detailed description of the exact product being procured (for example, 'Building a 200 meter bicycle path in Bergen'), which we use to classify all procured products into 54 different categories (see Appendix B.2). On product valuations, the data provide information about Public Roads pre-announced estimate of the NOK value of the contract (see Appendix B.1 for details on

presents more details on Public Roads procurement and the regulation of government procurement in Norway.

[5]The raw data comprises scanned versions of all procurement protocols archived by Public Roads for this period. As explained in Appendix B, after a procurement auction, the Public Roads procurer is required to archive a procurement protocol detailing every step in the procurement process. We hand-collected relevant variables from the procurement protocols. Machine-reading was not possible because the protocols are not standardized and in many cases are hand-written, causing an unacceptably high machine-reading error rate.

how Public Roads form this estimate). Finally, on contract duration, the data tell us about the procurement contract signing date and the expected contract completion date.

3.2 Register data

We use an extended version of the register data in Hvide and Jones (2018), who provide detail on the data. We use accounting information from the Dun and Bradstreet database of accounting figures based on annual financial statements submitted to the Norwegian tax authorities. These data include variables such as 5-digit industry code, sales, assets, number of employees, and firm profits for the years 1992-2016.[6] We supplement these data with incorporation documents submitted by new firms to the Norwegian government agency Brønnøysundregisteret. This register includes the start-up year, capitalization, and the personal identification number and ownership share of all initial owners with at least 10 percent ownership stake.

We also use data on individuals from 1993 to 2015, prepared by Statistics Norway. These records are based on government register data and tax statements, and include anonymized personal identification numbers and yearly socio-demographic variables such as gender, age, education in years, taxable wealth, and income. The data contain all Norwegian individuals, not a sample as in the Panel Study of Income Dynamics (PSID) or the Survey of Consumer Finance (SCF). As with the PSID and the SCF, the data are anonymized (contains no names of individuals). Individuals in these data can be linked to their employers via firm identifiers, which allows us to identify all the workers of the startups in the sample.

We define a startup as a firm that is younger than 10 years and is not a subsidiary at birth. Moreover, to exclude a relatively small number of very large young firms from the startup sample, we define startups as having less than NOK 16 million ($\approx$ USD 2 million) in total assets in the first and second years of operation. Our results are not sensitive to using a higher

[6]The Dun and Bradstreet database contains yearly information on all Norwegian incorporated limited liability companies, and not a sample (as in the U.S. equivalent). Incorporated companies are required by law to have an external auditor certifying the accounting statements in the annual reports.

total asset threshold, such as NOK 30 million.

3.3 Name matching and sample restrictions

Firm identifiers allow us to link the databases described in Section 3.1–3.2. The procurement data do not contain firm identifiers, only firm names. To collect firm identifiers, we first match, by exact spelling, bidder names in the procurement sample to firm names in the Dun and Bradstreet database.[7] This procedure yields firm identifiers for about 80% of the bids. Second, for initially unmatched bids, we use a fuzzy name matching algorithm, which allows us to find proposed matches where spelling is not identical (e.g, due to spelling errors, or because Public Roads abbreviates firm names). To reduce the risk of false matches, we manually check all proposed matches from the fuzzy name matching to determine which are acceptable and which are not.[8] In total, we find firm identifiers for 88% of all the bids in the procurement data.

We impose several sample restrictions, all at the procurement auction level. First, as our empirical methodology in Section 4 is based on price-only auctions, we exclude all auctions where price is not the only winning criterion. Second, we exclude auctions that 1) are not publicly announced or 2) are not open for all interested bidders (see Appendix B for more details on these auction types). Third, we exclude auctions with only one bidder. Fourth, we exclude auctions where we do not have bid sizes for all bidders. Finally, we only include auctions where we have successfully matched firm names to firm identifiers for either the winner or the runner-up. Of these restrictions, only using price-only auctions reduces the sample (by far) the most,

[7]Before the first round of name matching, we cleaned the bidder and firm names in several ways to maximize the match rate. For example, common words such as AS (the abbreviation for Norwegian limited liability company) and Byggmester (Norwegian for builder) were standardized between the procurement database and the Dun and Bradstreet database, and we cleaned several obvious spelling errors.

[8]Another potential source of false matches is that the same firm name can be associated with different firm identifiers. To reduce the risk of false matches, for all firms with identical or very similar spelling, we identify the firm that appears to be the best match in our setting. Specifically, we assign each firm a "match score" based on whether the firm is active in the correct period and whether the firm is in the correct industry, and keep the firm with the highest score. Given the composition of our sample, firms in the construction industry are the most likely matches, receiving the highest match score, followed by "Other business activities" and "Transport and telecom".

excluding around 4,000 of 10,500 auctions. The remaining restrictions reduce the sample by an additional 2,500 auctions, fairly evenly spread across the restrictions.

3.4 Summary statistics

Table 1 summarizes the final sample of procurement auctions and bidders. Panel A presents aggregate statistics. The sample comprises 4,083 procurement contracts with a total value of approximately NOK 125 billion. There are 1,505 unique bidders and 789 unique winning firms. Panel B of Table 1 presents summary statistics at the procurement-level. The median winning bid is NOK 6.66 million and the largest winning bid is NOK 2.3 billion. The average contract duration is 13 months and the average auction has 3.78 bidders. Estimated values and contract duration are missing in approximately 10% of the auctions.[9]

Panel C of Table 1 summarizes the firm-year level data for both startups and mature firms. The data comprise firm-year observations in the period 2003–2015 for all firms that place at least one bid in one of our 4,083 procurement auctions. On average, firms are 11 years old, have 81 employees, sell for NOK 177 million, and have assets worth NOK 132 million. There is significant skewness in all firm characteristics. For example, the median number of employees is 16 while the maximum is 25,507. Similarly, average assets are nearly ten times larger than median assets. The average firm-year sees 0.25 auction wins coming from 1.62 auction bids. The average auction winnings are 3.12 million and the maximum winnings are NOK 3.6 billion.

4 Empirical methodology

We aim to estimate the causal long-run effect of temporary demand shocks on firm outcomes. The identification strategy involves comparing procurement winners and marginal losers before and after procurement auctions. Section 4.1 explains how this strategy may yield causal effects.

[9] We impute contract duration to 12 months whenever the contract duration is missing. As shown in Table 1, the average contract duration is 13 months and the median contract duration is seven months.

Section 4.2 defines the estimation sample and Section 4.3 presents the regression model. Section 4.4 performs diagnostics on the main identifying assumption of the empirical strategy.

4.1 Outline of the empirical strategy

Suppose that J startups, indexed by j, each participate in one procurement auction, indexed by k, that takes place in year t. Let $\text{Winner}_j = \mathbf{1}(v_j < v_k^*)$ be an indicator for whether firm j wins its auction, which occurs when firm j's bid, v_j, is lower than v_k^*, the lowest bid among the other bidders in auction k. Furthermore, let Post_t equal 0 before the auction and 1 after, and let $b_{jt} = \text{Post}_t \cdot \text{Winner}_j$. For now, suppose we only have data from $\text{Post}_t = 1$. We can then express an outcome y_{jt} (e.g., sales four years after the auction) as:

$$y_{jt} = \alpha + \theta^c b_{jt} + \epsilon_{jt}, \tag{1}$$

where α measures the mean of y_{jt} for auction losers (i.e., when $b_{jt} = 0$), θ^c measures the difference in means of y_{jt} between auction winners and losers, and ϵ_{jt} represents all other determinants of y_{jt}, with $E[\epsilon_{jt}] = 0$. If $E[b_{jt}\epsilon_{jt}] = 0$, the coefficient θ^c in (1) captures the causal effect of an auction win.[10] In other words, if auction wins are uncorrelated with omitted determinants of y_{jt}, then b_{jt} approximates a random experiment. In reality, winning an auction may be correlated with other startup characteristics that influence y_{jt}, in which case $E[b_{jt}\epsilon_{jt}] \neq 0$. We deal with this issue in two ways. First, we compare winners to runners up only, in which case unobserved differences are likely to be smaller than if comparing auction winners to all auction losers. Second, we use a difference-in-difference formulation with fixed effects that yields a less restrictive identification assumption than $E[b_{jt}\epsilon_{jt}] = 0$, as explained below.

Suppose now that we can observe startups both before ($\text{Post}_t = 0$) and after ($\text{Post}_t = 1$) the

[10]Observe that if $E[\epsilon_{jt}] = 0$, then $E[b_{jt}\epsilon_{jt}] = 0$ implies a zero correlation between auction wins b_{jt} and the error term ϵ_{jt}. Specifically, if $E[\epsilon_{jt}] = 0$, then $\text{Cov}(b_{jt}, \epsilon_{jt}) = E[b_{jt}\epsilon_{jt}] - E[b_{jt}]E[\epsilon_{jt}] = E[b_{jt}\epsilon_{jt}]$. Observe further that assuming $E[\epsilon_{jt}] = 0$ is trivial as long as equation (1) includes an intercept, because a non-zero mean of ϵ_{jt} will be absorbed by the intercept term (e.g., Wooldridge 2013, p. 24).

auction. We can then modify eq. (1) into the following difference-in-difference model:

$$y_{jt} = \alpha + \theta^d b_{jt} + \beta \text{Post}_t + \omega \text{Winner}_j + \eta_{jt}, \tag{2}$$

where α now measures the mean of y_{jt} for auction losers in the pre-auction period (i.e., when $\text{Post}_t = \text{Winner}_j = b_{jt} = 0$). In contrast to the coefficient θ^c in equation (1), which measures the difference in y_{jt} between winners and runners up when $\text{Post}_t = 1$, the coefficient θ^d in equation (2) measures the difference in y_{jt} between winners and runners up when $\text{Post}_t = 1$ net of any differences in y_{jt} between these two groups that also existed when $\text{Post}_t = 0$.[11] Causal estimation of θ^d requires that $E[b_{jt}\eta_{jt}|\text{Post}_t, \text{Winner}_j] = 0$. In other words, conditional on pre-existing differences between auction winners and runners up, observed differences in y_{jt} in the post-auction period should entirely be driven by the auction win. This is commonly referred as the 'common trends assumption.' While the common trends assumption cannot be directly tested, Section 4.4 provides an indirect test of the common trends assumption by comparing the pre-auction trends and levels in y_{jt} for auction winners and runners up.

4.2 The treatment and control groups

In Section 4.1, we assumed that startups can participate in only one procurement auction. In the data, startups can compete in several auctions spread out over several years. One approach would be to use data on all the auctions and create treatment and control groups based on winners and runners up from each of the auctions (as Cellini et al., 2010, do in a related setting). However, if there are delayed effects of auction wins, previous winners would follow different

[11] If winners and runners up are identical in their pre-auction characteristics, equations (1) and (2) deliver identical conclusions regarding the effects of auction wins on startup outcomes (that is, $\theta^d = \theta^c$). Specifically, using conditional expectations, we have that $\theta^d = [E(y_{jt}|\text{Winner}_j = 1, \text{Post}_t = 1) - E(y_{jt}|\text{Winner}_j = 0, \text{Post}_t = 1)] - [E(y_{jt}|\text{Winner}_j = 1, \text{Post}_t = 0) - E(y_{jt}|\text{Winner}_{jt} = 0, \text{Post}_{jt} = 0)]$. Similarly, we have that $\theta^c = [E(y_{jt}|\text{Winner}_j = 1, \text{Post}_t = 1) - E(y_{jt}|\text{Winner}_{jt} = 0, \text{Post}_{jt} = 1)]$. Thus, if $[E(y_{jt}|\text{Winner}_j = 1, \text{Post}_t = 0) - E(y_{jt}|\text{Winner}_{jt} = 0, \text{Post}_{jt} = 0)] = 0$, it follows that $\theta^d = \theta^c$.

trends than their (non-winner) controls, biasing the results.[12] To circumvent this issue, we focus on firms that have never before won an auction. The treatment group comprises firms that win an auction for the first time. The control group comprises firms that are runners up and have not won an auction before.[13] We allow firms that come second in multiple auctions to enter the control sample multiple times, conditional on never before having won. In Appendix F, we show that the results are robust to only using firms' first auction.

Table 2 summarizes the estimation sample. The observations in Table 2 are at the startup-auction level, which means startups with multiple auctions are included multiple times. Startup characteristics are from the year before the auction in question. The estimation sample has 346 startups-in-auctions and 316 unique startups. The average startup is 3.83 years old and the oldest is eight years. The median startup has 7 employees while the average firm has 17 employees. On average, startups sell for NOK 24 million and have NOK 13.5 million in assets, with median sales and assets of NOK 13.4 million and 6.3 million, respectively. Startups participate on average in 1.3 procurement auctions and win 0.58 contracts. The bid size conditional on winning is NOK 11 million, and the median bid size is NOK 3.63 million. As expected, startups are smaller than the average firm in our overall sample (see Table 1).

[12] This would involve a violation of the common trend assumption. For example, suppose that a firm wins one auction in 2002 and another in 2004. Suppose further that the effect of each win is a 20% increase in sales after four years, materialized in 5% yearly increments. As control firms for the 2002 and 2004 wins, we would use runners up in 2002 and 2004, respectively. For simplicity, assume that the winner and both sets of runners up would have zero sales growth absent auction participation. Using only data from the 2002 auction, we would recover a causal effect because the auction winner and the runners up have the same zero-growth counterfactual trend. Focusing on the 2004 auction, however, the winner still experiences a 5% yearly sales growth from the 2002 win while the runners up have zero growth, which violates the common trends assumption. The empirical approach in our paper is akin to only using the 2002 auction.

[13] A small number of startups come first or second multiple times within the same calendar year. Throughout the empirical analysis, we define treated firms as those that win at least one auction and control firms as those that come second at least once but do not win. In unreported analyses, we modify in two ways the baseline model to accommodate multiple auction participations and wins. First, we replace the win indicator b_{jt} in equation (2) with a variable Wins_{jt} that counts the number of auction wins in a given treatment year. Second, we interact the time dummies in equation (2) with Partic_{jt}, a variable that counts the number of auction participations. This allows us to compare two firms that participate in the same number of auctions, where one firm wins at least one auction and the other firm loses all its auctions. The extended model delivers results almost identical to the baseline model.

4.3 Regression model

We now generalize equation (2) to accommodate a richer set of fixed effects as well as the possibility for multiple auctions. First, we replace Post_t with event-time fixed effects, where event-time e is centered on the auction year. Second, we include calendar year fixed effects to capture business cycle variations. Third, to accommodate that firms may enter the sample in multiple auctions, we follow Cellini et al. (2010) and Lafortune et al. (2018) and include a separate copy of the firm's time series surrounding each auction, and replace Winner_j in (2) with firm-by-auction fixed effects.[14] We estimate the following regression:

$$y_{jek} = \theta b_{jek} + \kappa_t + \alpha_e + \lambda_{jk} + \varepsilon_{jek}, \tag{3}$$

where y_{jek} is the outcome for firm j in event-time e centered on auction k, and κ_t, α_e, and λ_{jk}, are calendar-time, event-time, and firm-by-auction fixed effects, respectively. θ measures the post-auction difference in y_{jek} between winners and runners up net of pre-auction differences, and has a causal interpretation if winners and runners up would follow the same trend in y_{jek} absent the auction. We cluster standard errors at the firm-level to adjust for dependence created by the use of duplicate firm-year observations or by serial correlation in the error term ε_{jek}.

In an ideal experiment, both treatment and control firms would participate in exactly one auction. In reality, however, both treatment and control firms may choose to participate in more than one auction and their decision to do so may be influenced by the outcome of the initial auction. While θ in equation (3) remains a causal effect, its interpretation differs from the causal effect if firms can participate in only one auction, denoted θ^{Hyp}. Whereas θ in (3) captures both a) the effect of the initial auction win and b) the effects that operate through firms'

[14]The majority of firms in our sample participate in only one auction, and for these firms the firm-by-auction fixed effects λ_{jk} are identical to firm fixed effects (which would simply be denoted by λ_j). However, if (for example) a firm participates in and loses auctions in the calendar years 2003 and 2005 before winning an auction in 2010, that firm is represented in the sample three times — twice as control firms for the 2003 and 2005 auction winners, and once as a treated firm for the 2010 auctions — each time with a different λ_{jk}.

subsequent auction participation, θ^{Hyp} would only capture the effect of the initial auction win (in Cellini et al. 2010, θ and θ^{Hyp} are called 'intention-to-treat' and 'treated-on-the-treated' effects, respectively). In the current paper, we are interested in the total effect on startup outcomes of winning a procurement auction, which is captured by the coefficient θ in (3).

4.4 Assessing the identifying assumption

As explained in Section 4.1, the identifying assumption for the estimated θ in equation (3) is that auction winners and runners up would follow the same trends in their outcomes absent the procurement auction. To assess whether the identifying assumption is plausible, we compare the levels of trends of winners and runners up in years before the focal auction. The results are presented in Panel A of Figure 2. The figure shows that winners and runners up are indistinguishable in both levels and trends of key firm characteristics before the focal auction. While the identifying assumption only requires the balance of trends, we find it reassuring that both levels and trends are balanced between auction winners and runners up.

As a 'placebo' exercise, we compare the characteristics of runners up and *third-placed* startups in periods before and after the focal auction. If ranking second and ranking third is associated with true underlying differences, we would expect these two group of firms to have different trends after the auction. Conversely, if differences in ranking reflects randomness, which is the essence of our identification strategy, there would be no or small such differences. In Panel B of Figure 2, we show that runners up and third-placed firms are similar in both levels and trends of their pre-auction characteristics. In Appendix D, we estimate equation 3 using runners up as the 'treatment' group and third-placed firms as the 'control' group, and find economically and statistically insignificant estimates of the treatment effect, θ.

5 Main results

Table 3 shows the estimated effects of auction wins on startup outcomes, in the contract and post-contract period, respectively.[15] In the contract period, we find statistically and economically significant differences in sales, value added, and employees between the treatment and control group startups. These effects persist in the post-contract period. The post-contract coefficients suggest that winners have 24% higher sales and 22% more employees than runners up in the longer run. Moreover, we find an increase in profitability significant at the 5% level. In column 5, we find no difference in the post-contract survival rate between winners and runners up.[16] In column 6, we show that the increase in the total wage bill (which includes executive compensation) is of similar magnitude to the increase in employment. Finally, column 7 shows that physical capital, measured by tangible assets, increases by about 38%.

The split between the contract and post-contract periods in Table 3 is fairly crude and may disguise long-run convergence between auction winners and runners up. In Panels A and B of Appendix Figure A.VI, we plot raw means of Log(Value added) and Log(Employment) for winners and runners up over event-time, where event-time is measured relative to the contract duration (for example, the tick '2yr after' uses data from two years after the contract was completed), and in Panels C and D, we plot the difference between winners and runners up with corresponding confidence intervals. Overall, Panels A–D in Figure A.VI show large effects of auction wins on firm size even several years after the end of the contract period.

Are long-run effects of auction wins unique to startups? In Figure 3, we estimate the long-

[15] The data on contract duration are collected from procurement protocols that are written before the procured product is delivered. As a consequence, there could be measurement error in our contract duration variable. Public Roads officials inform us that, due to fines for late performance, substantial deviations between the expected and actual contract duration are rare. For example, a Public Roads white paper (Public Roads 2019) shows that across a sample of procurement contracts completed in 2017, the average delay compared to the expected contract duration was only about two months. In Figure A.VI and Appendix Figure A.VI, we show that the effects of procurement wins on value added and employment persist several years after the assumed contract duration.

[16] We define firms as being inactive in two steps. First, a firm is inactive in a given year if it is not reported in the Dun and Bradstreet database of accounting figures (see Section 3 for a description of this database). Second, a firm is inactive if both 1) sales are zero and 2) sales remain zero until the firm drops out of the Dun and Bradstreet database. In this latter case, firms are defined as inactive from the first year with zero sales.

run effects of auction wins, using a sample that includes both startups and mature firms. The effect of auction wins decreases sharply in firm age, becoming economically and statistically insignificant for firms aged above 10. The results in Figure 3 could follow from startups being quite small rather than startups being young. In Appendix E, we use a matching procedure to identify mature firms with comparable levels of revenue, employment, and assets as startups, and estimate the effects of auction wins for these "pseudo-startups". For similar-sized auctions as startups, in Appendix E, we find no long-run effect of auction wins for "pseudo-startups", suggesting that long-run effects of temporary demand shocks are unique to startups.

As explained in Section 4.1, for the difference-in-differences estimates in Table 3 to have a causal interpretation, we do not need that auction winners and runners up are balanced in the levels of their observable and unobservable characteristics, but we do need that winners and runners up are balanced in the trends of their characteristics. It could be that auction winners and runners up that place more similar bids also follow more similar trends, in which case restricting attention to more closely-contested auctions may allow for a cleaner identification. We therefore re-estimate the difference-in-differences model excluding auctions with fewer than three bidders (Table A.V) and restricting attention to the subset of auctions where the difference between the winner and runner-up bid is smaller than 10% (Table A.VI). Though statistical precision is reduced, the estimates in both cases are similar to those reported in Table 3.

The estimation sample used in Table 3 is over-represented by construction firms. To assess external validity, in Appendix I, we weigh our main regression to match the industry composition of the population of Norwegian startups, using a similar approach as Akerman et al. (2015). The resulting coefficient estimates, reported in Table A.VII of Appendix I, are very similar to those reported in Table 3. The standard errors are naturally larger. As another way to assess external validity, we can compare our estimates to those documented in the existing macroeconomic literature. Moreira (2016) uses longitudinal data on all firms in the United States between 1976 and 2011 to study the impact of firms' initial economic conditions (stage in the business cycle) on later firm size, as measured by employment. Moreira (2016) finds that a 1% deviation of GDP from its trend is associated with a 1% increase in the average size of the firms born in that same year. A similar elasticity of early-life demand to later-life size can be obtained in our setting by taking the ratio of the post-contract employment effect to the contract-period effect on sales (i.e., the initial demand shock). Doing so, our causal estimates suggest an elasticity of 0.88, broadly similar to the estimate in Moreira (2016).

Finally, the average causal effect of procurement auction wins reported in Table 3 could conceal heterogeneity depending on the value of the procurement contract. In Figure 4, we allow the effect of auction wins to vary with demand shock intensity, as measured by the contract value divided by firm sales in the year before the auction. We first sort auction winners into quintiles of demand shock intensity and then interact the contract-period and post-contract period indicators with a full set of dummies for demand shock intensity quintiles. Figure 4 plots the post-contract quintile effects for the natural logarithm of value added and employment. For small demand shocks (quintiles 1–3), we find negligible long-run effects. For larger demand shocks (quintiles 4–5), we find statistically and economically significant long-run effects.

Similar to the empirical approach in our paper, two working papers, Ferraz et al. (2016) nor Lee (2017), use procurement auctions data to empirically assess the long-run effects of temporary demand shocks in the form of auction wins. Neither has access to information on subsidiary status, which is needed to identify startups as opposed to spinoffs. Using data from Brazil, Ferraz et al. (2016) document positive effects of auction wins on firm growth that are temporary and die out after 2-3 years (Figure 4, p. 34). Compared to the procurement contracts in our paper, on average worth NOK 11.3 million, Ferraz et al. (2016) analyze much smaller contracts, their Table 1 reporting an average contract value of 10 314 Brazilian real ($\approx$ NOK 22 500), which could explain why the effects in their paper are only transitory. For the auctions with the lowest treatment intensity in 4, we do not detect long-run effects either. Using procurement auctions data from Korea, Lee (2017) studies the effects of auction wins for firms whose assets exceed \$10 million. These firms are clearly unlikely to be startups. Lee (2017) documents a positive long-run effect of auction wins on firm growth for firms aged two years or less but, puzzlingly, find negative effects for firms aged three and above (Table VIII, p. 36).

6 Startup dynamics

Section 5 shows that startups that win a procurement auction have significantly higher employment and sales than runner-up startups, even several years after the contract work is completed. The current section describes the financing, personnel, and product market choices that accommodate the winning startups' growth, — that is, *how* the startups expand. We use the same difference-in-difference technique as in Section 5 and compare changes in outcomes for startups that win a procurement auction to changes in outcomes for startups that narrowly lose an auction. In Section 7, we relate our findings to the existing theoretical literature and discuss likely mechanisms behind the main results — that is, *why* the startups expand.

6.1 Financing and Restaffing

Financing. We first address how startups finance the expansions documented in Section 5. During the contract period, the procurement demand shock generates increases in both debt and paid-in capital, 11% and 18% respectively, as shown in columns 1 and 2 of Panel A, Table 4. Only the increase in paid-in-capital is statistically significant. In the post-contract period, the effect on debt is 8% and the effect on paid-in capital is 28%, again only the latter being statistically significant. The leverage ratio, as shown in column 3, is significantly lower in the post-contract period than in the pre-auction period. These results suggest that debt is initially a quite important source of funding, while equity financing becomes more important in the longer run, which squares well with Robb and Robinson (2014) and Cole (2013).

Managerial quality and pay. We next address whether auction winners replace their manager, and to one with a higher quality in association with the expansion. As a proxy for managerial quality that may be exogenous to the auction outcome, we follow Kline et al., (2018) and use managerial wage in the year *before* the focal auction. A firm's managerial quality will increase (decrease) if the current manager is replaced with one that had higher (lower) wages in the year before the auction, and remain unchanged if the manager is not replaced. Estimating eq. (3) using firms that replace their managers only, we find an economically significant long-run increase in managerial quality (13%) for auction winners relative to runners up. However, as fairly few firms replace their managers, the standard errors are large and the effect is not statistically significant.[17] Using the full sample of firms, including those that do not replace their managers, the increase in average managerial quality is economically significant and borderline statistically significant both in the contract period (5% increase, $\text{pvalue} = 0.09$) and in the post-contract period (6% increase, $\text{pvalue} = 0.11$). Managers of winning firms have increased remuneration by about 8% in the contract period, and this effect increases to about 14% in the post-contract period, the latter being significant at the 5% level, as shown in column 6 of Table 4. In unreported regressions, we find that the post-contract increase in managerial pay holds both for firms that replace their manager and those that do not.

Worker quality and pay. We also explore whether startups increase the quality and pay of their workers during expansion. Analogous to the managerial quality analysis, we define worker quality based on workers' wages in the year before the focal auction, and investigate whether auction winners increase the quality of their workforce over time relative to runners up. Somewhat surprisingly, in column 7 of Table 4, we do not find any change in the average workforce quality. We also estimate equation (3) using the log of average worker wage (excluding the manager) as the outcome variable. In column 8 of Table 4, we find no economically or statistically significant effect of auction wins on average worker wages. Thus, winning startups appear to hire higher-quality managers but not higher-quality workers.

[17] In total, 37 startups replace their managers after the focal auction. Of these, 26 are winning firms while only 11 are runners up, suggesting that winning startups replace their manager at a higher rate than runners up.

6.2 Total factor productivity

In column 9 of Panel A of Table 4, we explore the effects of procurement auction wins on total factor productivity (TFP). In Table 4, we estimate TFP using the same approach as Bloom et al. (2013) and find no effects of auction wins on TFP (Appendix J provides further details on how our main TFP measure is constructed). Following Syverson (2011), in Appendix J we also explore the effects of procurement auction wins on two alternative TFP measures, and reach the same conclusion. With the caveat that firm-level TFP is notoriously difficult to estimate when, as in our case, the available data only contain firm-level aggregates such as total revenue, total number of employees, and total capital stock (Syverson 2011), we do not find direct evidence of startups increasing their TFP after winning a procurement auction.

6.3 Product market choices

The accounting data described in Section 3.2 do not contain information on customer relationships. However, the Public Roads auctions data detail firms' relationship with Public Roads after the focal auction (through activity in consecutive procurement auctions), which enables us to follow how startups' product market choices evolve after the focal auction. We are interested in whether startup expansions are associated with vertical movement into higher ends of the market or with horizontal movements into new markets, or both.

In Panel B of Table 4, we study whether auction wins affect whether firms choose to participate in subsequent Public Roads auctions, and which types of auctions they participate in. Column 1 shows that auction winners are 13 percentage points more likely to participate in another auction in the future compared to runners up.[18] On auction types, columns 2 and 3

[18]In unreported regressions, we show that the increase in Public Roads auction participation also translates into an increase in the number of Public Roads auction wins. However, auction winners do not become more reliant than auction losers on Public Roads demand. Decomposing firms' revenue into an Public Roads-related part and a 'private' demand part, we find that winning a procurement auction causes an increase in the fraction of Public Roads-related demand during the contract period. After the contract period, however, auction winners experience an increase in private demand, which drives the Public Roads-related fraction of demand down to levels comparable with runners up (Public Roads demand accounts for about 25% of overall revenue for both winners

show that winners are more likely to participate in large (above-median value) auctions and more likely to participate auctions that are larger than the focal auction. Moreover, in column 4, we find that winners are more likely to compete in auctions where the competing bidders have above-median total factor productivity, which we interpret as winners moving vertically into more demanding markets. In column 5, we find that winners are more likely to enter auctions for new products (relative to the focal auction product). Finally, column 6 shows that winners do not participate more often in auctions where product quality is a winning criterion.

7 Discussion

The current section provides an economic interpretation of the main empirical findings in Section 5 and Section 6. In Section 7.1, we review the main empirical findings. In Section 7.2, we discuss mechanisms in light of the theoretical literature. To shed further light on mechanisms, in Section 7.3, we collect and analyze case evidence from newspaper articles.

7.1 Review of results

Startups are vehicles of job creation and new ideas, but relatively little is known about startup heterogeneity. This paper investigates the long-term effects of temporary demand shocks, by comparing startups that win a procurement auction with startups that are runners up. Our empirical findings suggest that temporary demand shocks have several measurable effects on long-run outcomes. Winning startups are more than 20% larger than runners up in terms of both sales and employment even several years after the contract period, despite being very similar ex-ante. Long-run effects of temporary demand shocks appear to be unique to startups; we find no or minimal long-run effects on mature firm outcomes of winning an auction.

Overall, the empirical results suggest that an overlooked factor — temporary demand shocks,

and runners up in the post-contract period).

or "luck" — can have long-term effects for startups due to path dependence. Although this idea is not new to the entrepreneurship folkore, the Microsoft narrative in the Introduction being one example, our paper is novel in quantifying causal effects in a broad cross-section of startups.

Many governments, including among others the US and the UK, currently employ policies that facilitate the participation by startups and small firms in government procurement auctions, as a means to enhance startup creation and growth.[19] Our results provide a possible rationale for expanding such policies — in terms of job creation and sales growth, winning a procurement auction seems to have much larger effects for young firms than for mature firms.

7.2 Mechanisms

Why do temporary demand shocks have long-term effects on startup firm size? In a simple neoclassical model with a constant environment, no financial constraints, and no learning, a firm faces the same profit-maximization problem in each period, and a temporary demand shock would only give a temporary increase in firm size. Several theoretical models that deviate from this setting, however, suggest mechanisms through which temporary demand shocks could have long-term effects on firm size. We now review these theories and relate them to our empirical findings. Appendix L formalizes the theories in a simple unified framework.

Financial constraints. In the Evans & Jovanovic (1989) and Fan et al., (2017) models, startups have limited access to funding and may therefore be forced to operate at a suboptimal scale. A temporary demand shock — in our context, winning a procurement contract — can provide the startup with a stream of cash that allows for the financing of an expansion that

[19]For example, the US government limits competition for certain procurement contracts (for example those with an estimated value below $150,000) to small businesses, a policy intended to help small businesses compete for and win federal contracts, see https://www.sba.gov/federal-contracting/contracting-guide/types-contracts#section-header-0. In the UK, a major policy reform intended to open up government procurement contracts for small firms, including abolishing pre-qualification for all small contracts, see https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/402897/Lord_Young_s_enterprise_report-web_version_final.pdf. The Australian government has recently capped the maximum value of IT contracts, see https://www.afr.com/technology/government-reforms-it-procurement-in-650m-boon-for-local-startups-20170822-gy1hyf.

brings the startup closer to the optimal scale. Hence, relaxed financial constraints could drive the long-run effects of temporary demand shocks observed in Section 5.[20]

The relationship between cash holdings and financial constraints has been subject to a considerable debate. While it can be argued that financially constrained mature firms are likely to hold *more* cash as a precautionary motive (e.g., Almeida et al., 2004) it seems unlikely that young financially constrained firms have had the opportunity to hoard cash. Cash levels therefore appears to be a reasonable proxy for whether a startup is liquidity constrained or not. In Appendix M, we separate the startups in our sample into two subsamples; those with a high cash-to-assets ratios in the year before the focal procurement auction and those with a low cash-to-assets ratio, the idea being that firms with a lower cash holding are more likely to be financially constrained (Denis & Sibilkov, 2010) and therefore have greater effects of winning an auction. Somewhat surprisingly, the estimated treatment effects are *smaller* for the "cash constrained" group, which suggests that the relaxing of financial constraints through winning a procurement auction does not play an important role. This finding may suggest that cash-constrained startups have limited benefits from winning because they are unable to finance an expansion, and that financial aspects play a complementary role in driving the results.

Learning about oneself. In the Jovanovic (1982) model, firms start up with heterogenous productivity but are initially unaware of these differences in productivity. Gaining experience (in our context, from the contract work), startups learn about their own productivity, which leads those that receive positive news about own productivity to expand and those that receive negative news about own productivity to exit at a higher rate (Pakes & Ericson, 1998).

In our context, one would expect that auction winners learn more about their own productivity than do auction runners up during the contract period. This learning should, according to the Jovanovic (1982) and Pakes & Ericsson (1998) arguments, plausibly yield higher exit rates for auction winners than for runners up. In Panel A of Table 3, however, we find that auction win-

[20]Available evidence suggest that size differences based on differences in financial constraints can be long-lasting (e.g., Cabral & Mata, 2003).

ners do not have higher exit rates than runners up, neither during nor after the contract period. Thus we do not find strong support for the Jovanovic (1982) mechanism.

Demand frictions. Startups are initially a lesser-known entity and may need to reduce demand frictions through the development of customer relationships and a brand name (e.g., Foster et al., 2016). Such demand accumulation involves a positive feedback loop: more sales (in our context, winning an auction) reduces demand frictions and leads to increased future demand, which may sustain long-run differences between auction winners and runners up.

To evaluate the demand friction hypothesis, recall that Public Roads have two main procurement auction formats — auctions where price is the only winning criteria, and auctions where perceived product quality is one of the winning criteria. If auction wins reduce demand frictions vis-a-vis Public Roads, one would expect winners to increase their participation in future quality-criteria auctions, in which Public Roads may use their discretion to favor suppliers they already know.[21] As shown in Panel B of Table 4, however, winning firms are no more likely to participate in quality-criteria auctions than are runners up. With the important caveat that the data only allow us to evaluate demand frictions vis-a-vis Public Roads, there appears to be little direct evidence for the demand frictions hypothesis in the data.

Learning-by-doing. Arrow (1962) argued that underlying the production function of a firm is knowledge, and knowledge increases with experience. Similar to the learning-about-oneself and demand friction mechanisms, the learning-by-doing hypothesis involves a positive feedback loop: experience — in our context, by completing an Public Roads contract — enhances the firm's capabilities, which leads to more customers and more experience, resulting in size differences between winning and losing startups that persist even in the longer run.

The learning-by-doing hypothesis as it is usually formulated (see e.g., Arrow, 1962 or

[21] Implicitly, we assume that reductions in demand frictions vis-a-vis a customer always increases future demand from that customer. This assumption may not hold. For example, if a startup performed poorly in its first Public Roads job, Public Roads might be less inclined to accept that firm's bid in a future auction where product quality is a winning criteria. We do not have data on whether Public Roads are satisfied with their suppliers' performance, but note that publicized court cases with startups appear rare. Neither does the case evidence based on newspaper articles, reviewed below, suggest that Public Roads is broadly unsatisfied with the performance of startups.

Thompson, 2012) posits that learning-by-doing has decreasing returns — the more accumulated experience, the less learning at the margin.[22] In our context, we therefore expect learning-by-doing effects from contract work to be strongest for younger firms. In Figure 3, we plot the treatment effects across firm ages, and find that treatment effects are broadly monotonically decreasing in firm age. These findings are consistent with strong learning effects for the very youngest firms and smaller learning effects for the "middle-aged" startups.

Although we do not find direct evidence of own-firm TFP effects, we do find that auction winners tend to enter subsequent auctions with higher productivity of competitors, and to hire managers of higher quality, suggestive of a latent productivity increase. Winning startups also appear to broaden their capabilities, as evidenced by them moving into new product categories. Overall, it seems that winning startups boost their knowledge base through the contract work experience.

Sunk costs. If investments in human or physical capital are irreversible, a temporary demand shock could justify investments that make a firm more productive and larger in the longer run (e.g., Sutton, 1991, Dixit, 1992, Das et al., 2007). For example, if investing in a long-lived machine that persistently reduces marginal cost can be justified by the procurement contract work, and that machine has a low resale value — because of e.g., costs of dismantling, shipping, and installing it elsewhere — auction winners may be larger than runners up in the longer run even if their technologies were identical before the auction. The argument is not confined to physical equipment, but may also be applied to investments in the human capital of workers, investments in IT software, and so on. In Table 3, we do observe that winning firms make significant investments, as evidenced by a lasting increase in tangible assets. Outside individual cases, however, some reviewed in Section 7.3, it is difficult to assess what fraction of investments are sunk, which is a well-known problem noted by the literature (Sutton, 1991).[23]

[22]See e.g. Benkard (2000) for evidence that learning-by-doing effects are strongest when firms are young.

[23]The accounting data describes investments and assets but does not contain information on what type of equipment or machinery that is purchased. Neither does it contain information on training of workers, where similar sunk cost mechanisms could apply.

To summarize, we can quite conclusively rule out a number of mechanisms as driving our results: neither relaxed financial constraints, learning-about-oneself, nor reduced demand frictions seem to play a large role in explaining why auction winners are significantly larger than runners up in the long run. We argue that the observed long-run effects of temporary demand shocks for startups are driven by investments. These investments are "passive", through learning-by-doing, and "active", through investments that are later sunk, two mechanisms that could play together and reinforce each other. In the next subsection, we provide case evidence from newspaper articles that appear broadly consistent with this conclusion.

7.3 Case evidence from newspaper articles

To help inform mechanisms further, we collected newspaper articles that describe startups' experiences with winning a Public Roads procurement auction.

Sample selection: To assemble the newspaper case database, we used Atekst, an online news platform with exhaustive coverage of articles from local and national media outlets. We searched for articles covering a Public Roads procurement auction win, and kept articles describing wins by startups. This left us with 72 articles.[24] After excluding 33 articles that contain mere announcements of a contract win and two articles that describe legal disputes, we are left with 34 articles covering 27 startups. This gives a "finding rate" at the firm level of about 13%, as shown in Panel A of Figure 5. As expected, the finding rate is higher for larger contracts. This lack of representativeness can be viewed as an advantage, as the effects uncovered in the register data are disproportionately driven by the winners of the larger contracts (see Section 5). To avoid double-counting startups that are mentioned in multiple newspaper articles, for each of the 27 startups, we only keep the newspaper article that was published first.[25]

[24]As in the main analysis, we define startups as firms that are 10 years or younger in the auction year, was not a subsidiary at birth, and had less than USD 2 million in total assets in the first two years of operation. In the newspaper analysis, this gave a slightly larger universe of startups than in the main analysis, since we also kept startups that won auctions after 2015 (i.e., after our Public Roads auction data ends).

[25]Analogously, the estimation sample in Section 5 only includes startups' first auction wins. In the rare scenario

The vast majority of the sampled articles (more than 90%, as shown in Panel B of Figure 5) include an interview with the startup's manager or employees, conveniently giving first-hand evidence. Most of the newspaper articles (more than 80%, as shown in Panel C of Figure 5) are written just after the Public Roads auction, and focus on short-term operational challenges with the forthcoming contract work, such as staffing or investment needs, which limits the amount of information they contain about the longer-run evolution of the firm.

Linking to mechanisms: With these caveats in mind, we divided the articles into five categories corresponding to the five potential mechanisms discussed in Section 7.2.[26] This gave a natural categorization as very few cases had content that overlapped categories. The results are presented in Panel D of Figure 5. Out of the 27 newspaper cases, 12 (44%) mention *investments* in new equipment and 7 cases (26%) mention better utilization of existing equipment, so that a broad investment category picks up 19 (70%) of the cases. Outside individual cases, some reviewed below, it is difficult to judge which fraction of these investments will later become "sunk" and thus more likely to have long-run effects, but it is clear that investments are a high-order priority for the startup to deal with as a consequence of being awarded the contract. 5 (19%) cases highlight the *learning* experience expected by the startup, and 2 (7%) cases describe how winning a contract involves expanding into product areas that are new to the startup. Thus, with a broad definition of learning, 7 (26%) cases are included. On *demand frictions*, only one (4%) case mentions that the contract experience can be used as a reference to attract new customers later on. No newspaper articles mention that the contract will ease *financial constraints* (some cases describe the need to raise new capital to finance the build-up, consistent with the findings of the register data analysis). Also, no newspaper articles mention that the contract work will give an opportunity to startups to *learn about their own productivity*.

To summarize, 70% of the sampled newspaper articles mention investment effects of win-

where the same startup is mentioned in multiple articles within the same year, we choose a random article.

[26]For robustness, we used a simple text recognition algorithm on the sampled articles and obtained broadly similar results. These results are available upon request.

ning a Public Roads auction; 26% mention learning effects; 4% mention demand friction effects; and none of the articles mention relaxed financial constraints or learning-about-oneself effects from winning an auction. Thus, consistent with the discussion in Section 7.2, the newspaper case evidence points to investments broadly — passive as in learning-by-doing and active as in buying new equipment — as key drivers of the long-run effects in the register data.

Case summaries: One example of a startup *investing* after winning a Public Roads procurement auction is Norsk Bergsikring, which specializes in securing roads from stone and mud avalanches by building protective fences. In a 2016 interview, Roy Sævik, the CEO of Norsk Bergsikring, describes how winning a Public Roads procurement contract has enabled the firm to invest in a highly specialized Menzi Muck climbing machine — of which according to Sævik "there are only a handful in Norway" — that can work in especially steep and challenging terrain. In another newspaper article, Henry Ringvold, the CEO and majority stakeholder of Dokka Entreprenør (according to the article, 85% of their revenue comes from Public Roads contracts) states that as a direct consequence of a Public Roads contract, Dokka Entreprenør has invested NOK 14 million ($\approx$ USD 2 million) in new machinery such as excavators, thus expanding to a total of 50 machines. These two cases illustrate heterogeneity in the degree of "sunkness": excavators are standard equipment for road builders and should be a rather liquid asset, while a specialized climbing machine would likely be more difficult to resell.

On *learning-by-doing*, Holdahl Maskin and Transport won a major contract on road maintenance in 2015. In an interview, CEO Levi Holdahl describes how the contract work entailed learning-by-doing effects on their planning and risk analysis abilities, facilitating future work. A different startup, that experienced both investment effects and learning-by-doing effects from winning a Public Roads contract, is the construction company Frosta Entreprenør. Founded in 2010, Frosta Entreprenør won a Public Roads contract in 2013 to move a stretch of road away from agricultural land. One of the firm's truck operators explains in an interview that the firm has acquired a specialized Hitachi 210 truck for the job. A key innovation with the Hitachi

210 truck is a sophisticated and very precise GPS system that warns the truck operator of any deviations exceeding 1 centimeter from the Public Roads blueprint. During the Public Roads job, the truck operator explains in one of the newspaper articles, he will acquaint himself with the new Hitachi truck which in turn will increase productivity in future jobs.

Also related to learning-by-doing, several newspaper cases describe how a Public Roads contract allowed the startup to move into new product categories, consistent with an enhancement of the firm's knowledge and capabilities. For example, Paneda specializes in DAB transmission in road tunnels — of which there are exceptionally many in Norway — and won its first Public Roads contract in 2013. Paneda has since developed complementary products to the DAB transmitter; a 2016 newspaper article describes winning a contract for a monitoring system that ensures that the transmitters are operative 24/7, and a current R&D effort to develop a future DAB-headend system. Similarly, Betongpartner AS — originally a specialist in the construction of ferry docks — describes in a newspaper article that in tandem with winning a Public Roads procurement contract, the firm is undertaking major investments to transition into tunnel construction work, which requires related but distinct technical expertise.

Finally, on *reduced demand frictions*, Orbiton AS specializes in photo and video monitoring of bridges using drone technology. Two years after its inception, in 2015, Orbiton landed a major Public Roads contract involving the monitoring of more than 300 bridges nationally. The CEO of Orbiton, Thomas Moss, states in an interview that Orbiton will use the Public Roads contract as a reference in their marketing towards other potential customers.

8 Conclusion

In this paper, we have assessed the effects of temporary demand shocks on long-term startup outcomes, by comparing startups that win a procurement auction with startups that are runners up. The main empirical finding is that these temporary demand shocks have large long-run effects on firm size — startups that win a procurement auction are more than 20% larger in terms of sales and employment than startups that narrowly lose an auction, even several years after the end of the contract work. The empirical analysis further suggests that a broad notion of investments, encompassing both learning-by-doing and sunk costs, is an important driver. The results are unique to startups: we do not observe long-run effects of auction wins for mature firms, even among mature firms that are comparable with startups in size.

Our work primarily informs a large academic literature (reviewed in the Introduction) that explores factors that may explain why some startups do better than others. We highlight that an overlooked factor, temporary demand shocks, can play an important role in shaping the long-term outcomes of startups. In contrast to much of the existing literature, we exploit exogenous variation in a broad cross-section of startups, allowing for causal statements. In terms of policy implications, many countries including the US and UK employ policies that aim to enhance startup growth through the allocation of government procurement contracts to young firms. Our results provide a possible rationale for expanding such policies: winning a procurement contract appears to have much larger long-run effects for startups than for mature firms.

9 References

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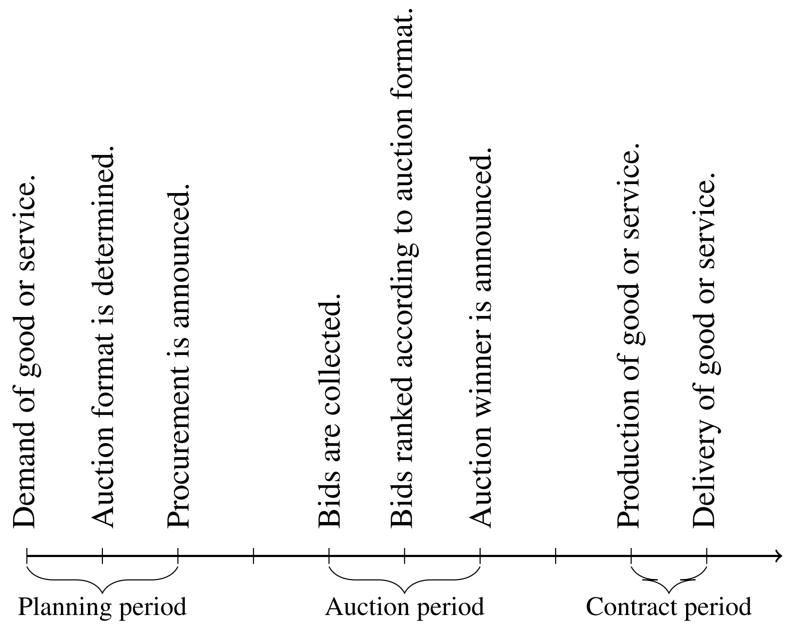
10 Figures

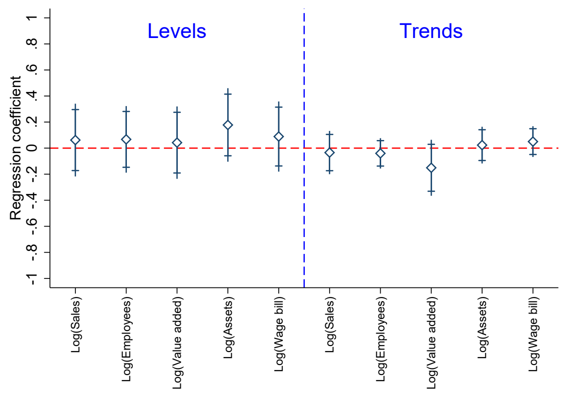
Figure 1: Timeline: Procurement process in Public Roads



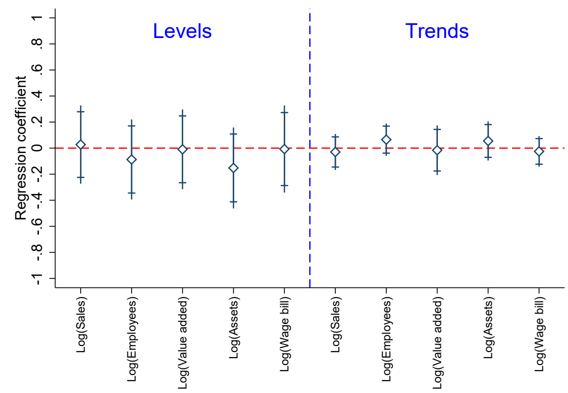
Note: The figure presents a timeline over the procurement process in the Norwegian Public Roads Administration. Procurement in Public Roads can be split into three separate periods: the Planning period, the Auction period, and the Contract period. In the Planning period, Public Roads determines the auction format and announces the auction format to the public. In the Auction period, bids are collected from private suppliers and ranked according to the auction format. Finally, in the Contract period, the auction winner produces and delivers the product. Our data on procurement protocols, described in Section 3, are collected immediately after the Auction period.

Figure 2: Assessing the identifying assumption

(a) Winner vs. runner-up

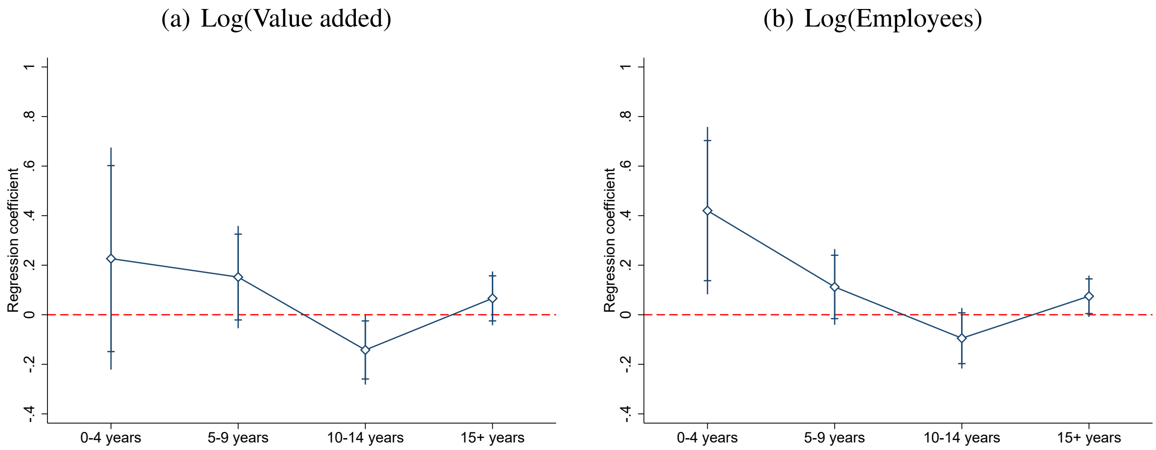


(b) Runner-up vs. third place



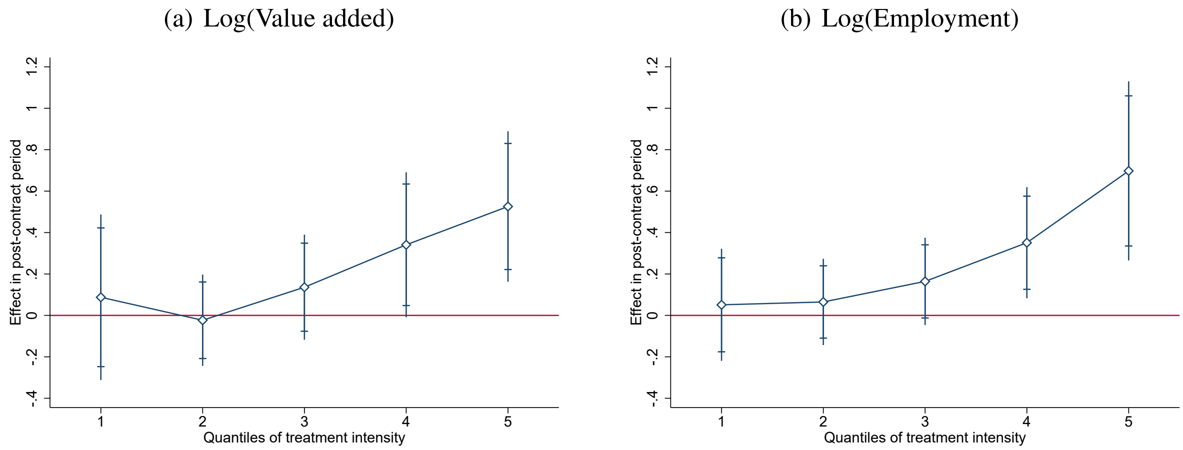
Note: In Panel A, we report tests of covariate balance between procurement auction winners and runners up, while in Panel B, we report balance tests between runners up and third-placed firms. In both panels, we report estimates from separate regressions where a pre-auction firm characteristic is regressed on a dummy for whether the firm wins a procurement auction in event time zero (the auction year). Firm characteristics are measured in levels and trends. For levels, we use all observations between event time -5 to -1 and include event time and calendar time fixed effects in all regressions. Trends are measured as the change in a given firm characteristic between event time -2 and -1. The outcomes considered are Log(Sales); Log(Number of employees); Log(Value added); Log(Assets): and Log(Total Wage bill). Standard errors are clustered at the firm level.

Figure 3: Effect on Log(Value added) and Log(Employees), by age bins



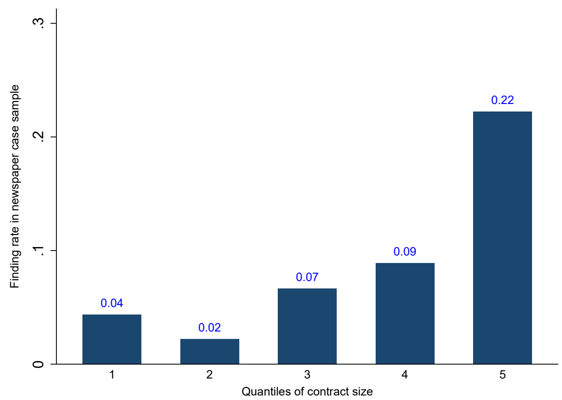
Note: The figure reports estimates of the post-contract treatment effect of procurement auction wins on Log(Value added) and Log(Employees). The post-contract treatment effect has been estimated separately for firms aged 0–4 years, 5–9 years, 10–14 years, and 15+ years in the auction year, using the following regression specification within each of the age bins: $y_{jek} = \theta b_{jek} + \kappa_t + \alpha_e + \lambda_{jk} + \varepsilon_{jek}$, where y_{jek} is the outcome for firm j in event-time e centered on auction k; b_{jek} equals one for auction winners in the post-auction period, zero otherwise; and κ_t, α_e, and λ_{jk} are calendar-time, event-time, and firm-in-auction fixed effects, respectively. The coefficient θ is allowed to differ in the procurement contract-period and post-contract period and, in the current figure, we only report the estimates of θ from the post-contract period. Standard errors are clustered at the firm-level. The figure reports 95% and 90% confidence intervals around the coefficient estimates.

Figure 4: Log(Value added) and Log(Employees), by treatment intensity

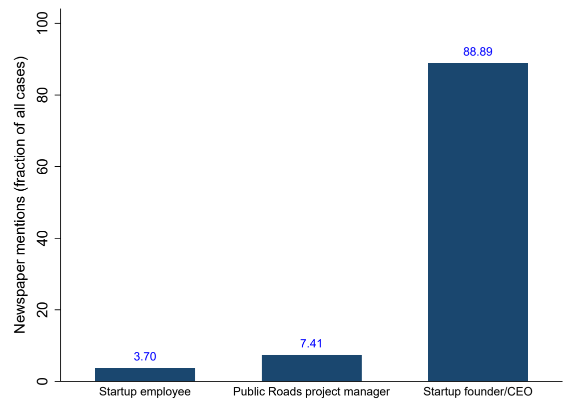


Note: The figure reports estimates of the post-contract treatment effect of procurement auction wins on Log(Value added) and Log(Employees). The post-contract treatment effect has been estimated for quintiles of treatment intensity using the following regression specification: $y_{jek} = \theta b_{jek} \times \text{Quant}_{jk} + \kappa_t + \alpha_e + \lambda_{jk} + \varepsilon_{jek}$, where y_{jek} is the outcome for firm j in event-time e centered on auction k; Quant_{jk} are quintiles-level fixed effects; b_{jek} equals one for auction winners in the post-auction period, zero otherwise; and κ_t, α_e, and λ_{jk} are calendar-time, event-time, and firm-in-auction fixed effects, respectively. Treatment intensity is measured as the total procurement auction winnings in event time 0 divided by firm sales in event time -1. The coefficient θ is allowed to differ in the procurement contract-period and post-contract period and, in the current figure, we only report the estimates of θ from the post-contract period. Standard errors are clustered at the firm-level. The figure reports 95% and 90% confidence intervals around the coefficient estimates.

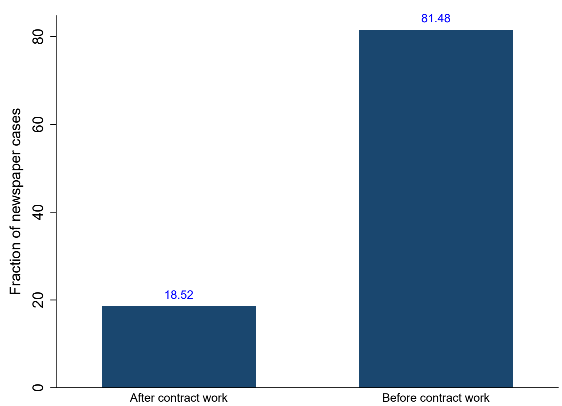
Figure 5: Summary statistics: Newspaper cases



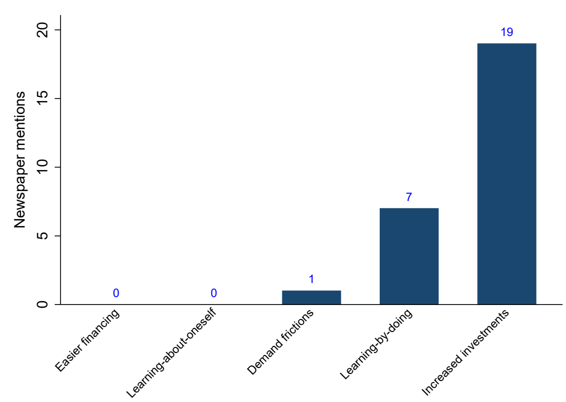
(a) Finding rate by contract size



(b) Interviewees



(c) Timing of newspaper article



(d) Mechanism mentions

Note: The figure presents summary statistics from our database of newspaper articles describing startups' experiences with winning Public Roads procurement auctions. After excluding newspaper articles that simply announce a contract win or describe legal disputes, the newspaper database comprises 27 articles covering 27 unique startups. In Panel A, we plot the 'finding rate' — the number of firms in the newspaper sample divided the number of startup winners in the estimation sample — by quintiles of Public Roads contract size. Contract size is measured as the NOK value of the contract divided by firm sales in the year before the auction. In Panel B, we summarize the interviewees in the 27 newspaper articles (the bars in Panel B sum to 100% because all of the sampled newspaper articles include an interview). In Panel C, we summarize whether the newspaper articles were written before or after the Public Roads contract work had started. In Panel D, we plot the count of newspaper articles that mention each of the five main theoretical mechanisms (investment, learning-by-doing, demand frictions, learning about oneself, and relaxed financial constraints), where the assigning of articles into different mechanisms is made based on our own reading of the articles.

11 Tables

Table 1: Summary statistics

Panel A: Aggregate statistics

Bidders	Winners	Contracts	Value (mNOK)
1505	789	4083	125478

Panel B: Procurement-level data

	μ	σ	Min.	p(25)	Median	p(75)	Max.	*N*
Auction year	2010.44	2.89	2003.00	2008.00	2011.00	2013.00	2015.00	4083
Contract duration (mths)	13.66	16.79	0.00	5.00	7.00	13.00	132.00	4083
Fraction construction	0.88	0.33	0.00	1.00	1.00	1.00	1.00	4083
Fraction services	0.10	0.30	0.00	0.00	0.00	0.00	1.00	4083
Fraction materials	0.02	0.15	0.00	0.00	0.00	0.00	1.00	4083
Number of bidders	3.78	1.77	2.00	2.00	3.00	5.00	16.00	4083
Winning bid (mill.)	30.73	104.71	0.00	2.58	6.66	17.14	2298.32	4083
Estimated value (mill.)	42.66	269.45	0.06	3.10	8.20	25.00	15000.00	3743

Panel C: Firm-year-level data

	μ	σ	Min.	p(25)	Median	p(75)	Max.	*N*
Firm characteristics								
Firm age	11.26	6.42	0.00	6.00	12.00	16.00	23.00	16125
Number of employees	81.41	476.07	0.00	6.00	16.00	38.00	25507.00	16125
Sales (mill.)	177.73	2471.79	-7.69	8.23	23.39	66.30	246463.71	16125
Total wage bill (mill.)	44.98	533.98	-5.87	2.49	6.90	18.08	63760.88	16125
Capital stock (mill.)	23.43	258.27	0.00	0.47	1.92	6.15	23338.95	16125
Total assets (mill.)	132.00	1953.87	0.00	5.18	13.62	39.01	204285.37	16125
Auction outcomes								
Number of wins	0.25	1.69	0.00	0.00	0.00	0.00	77.00	16265
Number of bids	1.62	4.71	0.00	1.00	1.00	1.00	214.00	16265
Any wins	0.12	0.32	0.00	0.00	0.00	0.00	1.00	16265
Total winnings (mill.)	3.12	52.86	0.00	0.00	0.00	0.00	3615.85	16265

Note: The table summarizes our procurement-level and firm-level data. Sample restrictions are described in Section 3.3. In Panel A, we summarize the number of unique bidders, winners, procurement contracts, and total NOK procurement volume in our sample of procurement contracts. In Panel B, we report summary statistics on key variables from the procurement-level data. The variables considered in Panel B are the procurement auction year; contract duration (in months); dummies for whether the procurement relates to construction, services, or materials; number of bidders; winning bid (in millions); and estimated contract value (in millions). For each procurement characteristic, we present sample averages (μ), standard deviations (σ), minimums, 25th percentiles, medians, 75th percentiles, maximums, and the number of observations (N). In Panel C, we report summary statistics from the firm-level data. In Panel C, the sample comprises firm-year observations in the period 2003–2015 for all firms that place at least one procurement auction bid between 2003–2015. We include all firm-year observations in 2003–2015, not only firm-year observations with procurement bids. The variables considered in Panel C are firm age; number of employees; sales (in millions); total wage bill (in millions); capital stock (in millions); total assets (in millions); number of procurement wins; number of procurement bids; a dummy for whether a firm wins a procurement in a given year; and the firm-year total procurement winnings.

Table 2: Summary statistics: Estimation sample

	Summary statistics							
	μ	σ	Min.	p(25)	Median	p(75)	Max.	N
	Panel A: Firm characteristics in year before auction							
Firm age	3.83	2.81	0.00	1.00	4.00	6.00	8.00	346
Number of employees	17.16	47.90	0.00	3.00	7.50	17.00	669.00	346
Sales (mill.)	24.19	37.89	0.00	5.53	13.44	26.89	394.04	346
Total wage bill (mill.)	7.30	18.47	-0.00	1.16	3.68	7.44	175.14	346
Capital stock (mill.)	4.10	38.47	0.00	0.23	0.84	2.40	713.66	346
Assets (mill.)	13.51	47.05	0.00	2.86	6.32	12.59	814.56	346
	Panel B: Auction characteristics							
Focal auction year	2009.78	2.94	2003.00	2008.00	2010.00	2012.00	2015.00	346
Fraction construction	0.82	0.38	0.00	1.00	1.00	1.00	1.00	346
Fraction services	0.13	0.34	0.00	0.00	0.00	0.00	1.00	346
Fraction materials	0.04	0.20	0.00	0.00	0.00	0.00	1.00	346
Estimated value (mill.)	12.25	48.82	0.10	1.91	5.00	10.49	815.90	305
Number of bidders	3.90	1.81	2.00	3.00	3.29	5.00	16.00	346
Bid size (mill.)	9.90	40.43	0.00	1.79	3.82	8.24	718.88	346
Bid size if win (mill.)	11.33	52.60	0.00	1.30	3.67	7.89	718.88	199
Bid/Estimated value	0.97	0.40	0.00	0.77	0.92	1.09	3.15	305
Winner	0.58	0.50	0.00	0.00	1.00	1.00	1.00	346
Number of focal auctions	1.33	0.81	1.00	1.00	1.00	1.00	7.00	346

Note: The table reports summary statistics from the estimation sample, using data from the calendar year before the focal procurement auction. For firms that start up in the year of the focal auction, we use data from the auction year. In Panel A, we present summary statistics on firm characteristics. The variables considered are firm age; number of employees; sales (in millions); total wage bill (in millions); capital stock (in millions); and total assets (in millions). In Panel B, we present summary statistics of the focal auctions. The variables considered are the focal auction year; dummies for whether the firm bids for construction, services, or materials contracts; estimated (by the procurer) contract value (in millions); average number of bidders; bid size (in millions); bid size conditional on winning (in millions); bid size divided by estimated value; a win dummy; and the number of focal auctions. For each variable, we present sample averages (μ), standard deviations (σ), minimums, 25th percentiles, medians, 75th percentiles, maximums, and number of observations (N).

Table 3: Main results

	Firm outcomes						
	Log(Sales)	Log(Value added)	Log(Employees)	Profits	Active	Log(Wage bill)	Log(Tang. assets)
θ (Contract)	0.25***	0.19**	0.14*	582.85**	-0.00	0.14*	0.13
	(2.81)	(2.11)	(1.95)	(2.03)	(-0.05)	(1.88)	(1.12)
θ (Post-contract)	0.24**	0.19*	0.22***	813.44**	-0.01	0.23***	0.38***
	(2.34)	(1.95)	(2.99)	(2.22)	(-0.18)	(2.91)	(2.74)
Adj R^2	0.76	0.77	0.82	0.63	0.34	0.81	0.74
N	2945	2926	2776	2946	3761	2843	2783

Note: The table reports estimates of θ from the following regression: $y_{jek} = \theta b_{jek} + \kappa_t + \alpha_e + \lambda_{jk} + \varepsilon_{jek}$, where y_{jek} is the outcome for firm j in event-time e centered on auction k; b_{jek} equals one for auction winners in the post-auction period, zero otherwise; and κ_t, α_e, and λ_{jk} are calendar-time, event-time, and firm-in-auction fixed effects, respectively. The coefficient θ is allowed to differ in the procurement contract-period and post-contract period. The outcomes are Log(Sales); Log(Value added); Log(Employees); profits; a dummy for whether the firm is active; Log(Total wage bill); and Log(Tangible assets). Firm profits have been winsorized at the top and bottom 2.5%. Standard errors are clustered at the firm-level. t-statistics in parentheses. Stars (***, **, *) indicate statistical significance at the 10, 5, and 1 percent level.

Table 4: Other outcomes

Panel A: Capital structure, production technology, and managerial quality

	Log(Debt)	Log(Paid-in cap.)	Lev.	CEO quality$^{\text{Switchers}}$	CEO quality$^{\text{All}}$	Log(CEO wage)	Worker quality	Log(Avg. worker wage)	Log(TFP)
θ (Contract)	0.11	0.18**	-0.06**	0.10	0.05*	0.08	-0.02	-0.02	0.04
	(1.37)	(2.24)	(-2.07)	(1.00)	(1.74)	(1.00)	(-1.01)	(-0.95)	(0.87)
θ (Post-contract)	0.08	0.28**	-0.08**	0.13	0.06	0.14**	-0.02	-0.01	-0.05
	(0.87)	(2.55)	(-2.38)	(0.90)	(1.58)	(2.22)	(-0.89)	(-0.27)	(-1.17)
Adj R^2	0.78	0.82	0.47	0.72	0.89	0.59	0.76	0.78	0.62
N	2942	2891	2944	351	1604	1633	2263	2270	2627

Panel B: Later auction participation: Indicators for participation

	Competes	Large	Larger	High TFP	New product	Quality
θ (Contract)	0.05*	0.10***	0.01	0.09**	0.11***	0.00
	(1.76)	(2.76)	(0.66)	(2.53)	(2.89)	(0.02)
θ (Post-contract)	0.13***	0.07***	0.12***	0.12***	0.10***	0.01
	(3.74)	(3.02)	(4.21)	(4.10)	(3.11)	(0.59)
Adj R^2	0.53	0.32	0.28	0.35	0.30	0.22
N	3761	3761	3761	3606	3761	3761

Note: The table reports estimates of θ from the following regression: $y_{jek} = \theta b_{jek} + \kappa_t + \alpha_e + \lambda_{jk} + \varepsilon_{jek}$, where y_{jek} is the outcome for firm j in event-time e centered on auction k; b_{jek} equals one for auction winners in the post-auction period, zero otherwise; and κ_t, α_e, and λ_{jk} are calendar-time, event-time, and firm-in-auction fixed effects, respectively. The coefficient θ is allowed to differ in the procurement contract-period and post-contract period. In Panel A, the outcomes are Log(Debt); Log(Paid-in capital); Leverage; CEO quality in firms that switch CEOs in the post-auction period; CEO quality in all firms; Log(CEO wage); worker quality; Log(Average worker wage); and Log(Total factor productivity) measured using the Bloom et al. (2013) approach. As explained in Section 6.1., CEO and worker quality is measured as the CEO or worker's wage in the year before the focal auction. In columns 4–6, we use CEO quality and wage as the outcomes and only include in our estimation sample firms that have a CEO both before and after the focal procurement auction. In Panel B, the outcomes are indicator variables for whether a firm competes in a price-only auction; competes in a large (above median) value auction; competes in an auction that is larger than the focal auction; competes in auctions where the competing bidders are above the median in terms of TFP; competes in auctions for new products; or competes in auctions where price is not the only winning criteria. Standard errors are clustered at the firm-level. t-statistics in parentheses. Stars (***, **, *) indicate statistical significance at the 10, 5, and 1 percent level.

(For online publication)

Appendix to:

Do Temporary Demand Shocks have Long-Term Effects for Startups?

Hans K. Hvide and Tom G. Meling

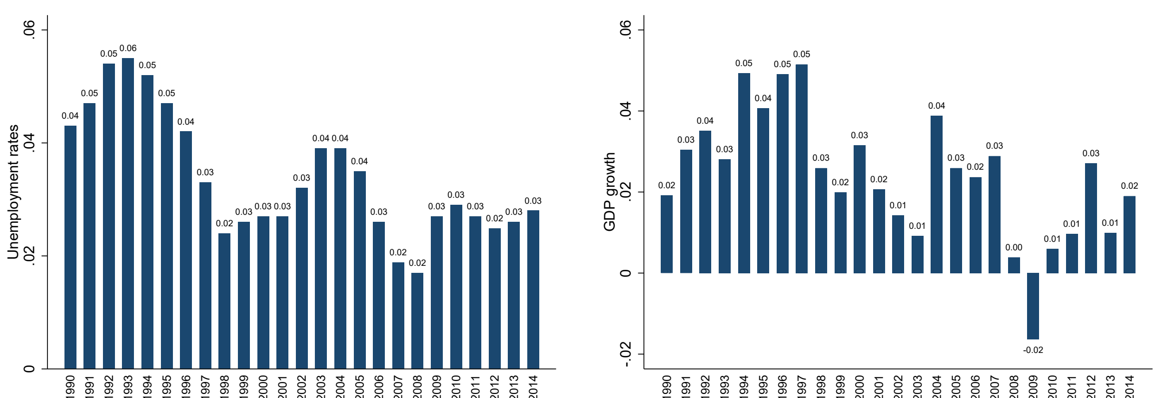
Table of contents:

A Business cycles in Norway

In the main text, we use data on government procurement auctions in Norway in the period 2003–2015 to explore the long-run effects on startup outcomes of temporary demand shocks. In the current appendix, we describe the aggregate economic conditions in Norway throughout the same time period. Between 2003–2015, Norway, like most other Western countries, experienced a boom-and-bust business cycle. Between 2001–2006, Norwegian real gross domestic product (GRP) growth averaged 3%. During the 2007–2009 financial crisis, however, Norway experienced negative GDP growth (in 2008) followed by a prolonged period of slow growth. Unemployment rates in Norway declined from 6% in 1993 to 3% in 2014.

Figure A.I: Aggregate economic conditions, 1992–2014

(a) Unemployment rates (b) Real GDP growth



Note: The figure presents summary statistics on aggregate economic conditions in Norway between 1990 and 2014. In Panel A, we plot yearly unemployment rates. In Panel B, we plot yearly real GDP growth. Data on aggregate economic activity have been collected from Statistics Norway.

B Procurement legislation and Public Roads procurement

The current appendix has two parts. In Section B.1, we describe the various laws that regulate government procurement in Norway. In Section B.2, we provide extra details on the procurement process in the Norwegian Public Roads Administration (Public Roads).

B.1 Procurement legislation in Norway

This note provides an overview of the current Norwegian procurement legislation. As the legal framework on procurement is highly complex and continually changing, the note is not exhaustive. We draw heavily on Regjeringen (2018), a government-written guide to Norwegian procurement law and regulation based on the legislation in place as of January 1, 2017 (the Regjeringen (2018) guide is only available in Norwegian). Government procurement in Norway is regulated by the Public Procurement Act and its accompanying regulations, most importantly the Public Procurement Regulation. As Norway is a member of European Economic Area (EEA), Norwegian procurement legislation is harmonized with EU procurement legislation.

The Public Procurement Act applies to the procurement of products, services, and construction work with an estimated value exceeding NOK 100,000, excluding value-added tax. All state governments, county and municipal authorities, statutory bodies, and associations between any of the aforementioned agencies, are bound by the Public Procurement Act. The procurement legislation is divided into separate parts depending on the procurement value. The first key threshold is NOK 100,000, where the basic regulations of the Public Procurement Act take effect. The second threshold is NOK 1.3 million, where the regulation of the procurement process becomes increasingly detailed. Finally, the so-called EEA thresholds, where procurements must be announced to suppliers Europe-wide, exceed NOK 1.3 million but vary by the product procured.[27] The threshold values are revised every second year

[27] For example, for procurement relating to construction services, the EEA threshold value is NOK 51 million, while for health and social services the EEA threshold is NOK 6.95 million.

As the applicable legislation depends on the procurement value, procurers must form an estimate of the expected value of the procurement. The procurement legislation provides strict guidelines for how this estimation should be carried out. First, the procurement value should be estimated based on the total cost of the procurement including, for example, potential add-ons, and excluding value-added tax. To form an estimate, the procurer is permitted to survey the market by interacting with potential suppliers and the procurer may seek expert advise from independent sources (e.g., consultants). However, a key restriction is that the estimation procedure may not interfere with competition at later stages by, for example, giving an unfair advantage to some suppliers over others. Finally, the procurer may not strategically split a procurement into smaller pieces to circumvent the legislation's threshold values.

The Public Procurement Act is guided by five fundamental principles: competition, equality, predictability, accountability, and proportionality. These principles are derived partly from EEA legislation and partly from Norwegian procurement legislation. Overall, the purpose of the five fundamental principles is to ensure efficient procurement. According to Norwegian procurement legislation, an efficient procurement process takes into account, among other things, competition; economic crime; and environmental, climate, and social issues.

From the principle of competition, as a main rule, all Norwegian procurement should be competitive. The principle of competition applies to procurement both above and below the legislation's threshold values. Above the threshold values, competition is promoted by mandating the public announcement of procurement auctions. Procurements below the EU threshold are announced on the Norwegian Doffin web site, while procurements above the EU threshold are announced on both Doffin and the EU TED web site. By announcing the procurement, the procurer reaches a deeper market, which can increase the number of bidders and reduce the cost of procurement. Below the threshold values, competition is promoted by announcing the procurement auction to "a reasonable number of suppliers". This can, for example, be achieved by a voluntary announcement (on Doffin and/or TED) of the procurement. Only on rare occasion are procurers allowed to deviate from the principle of competition and award a contract directly to a supplier (for example, there might only be one qualified supplier).

The Public Procurement Act's principles of transparency and accountability imply that procurers must be able to document all major decisions involved in the supplier selection process. In practice, these principles involve keeping written accounts of all assessments, communications, and decisions relating to each procurement, and making this documentation easily accessible to a third-party reader (this extensive documentation is the basis for our data in Section 3). As a main rule, all documents relating to government procurement should be publicly available, promoting scrutiny of the procurement process. In practice, however, many procurement documents are exempt from public view or are censored to protect supplier trade secrets (importantly, our data in Section 3 are not censored in any way).

When organizing procurement auctions, the Public Procurement Act gives procurers discretion over certain auction format features. Perhaps most importantly, the procurer chooses the winning criteria. However, the Act sets a number of requirements for the choice of winning criteria. First, all winning criteria should be objective, factual, and in accordance with the Act's five fundamental principles. Second, the winning criteria should be clearly stated in the procurement announcement and be easy to interpret by all interested suppliers (that is, the auction format should be predictable). Finally, the winning criteria should be designed in such a way that they do not unfairly favor one supplier over others.[28]

As a main rule, procurement auctions should be designed as either 'open competition' or 'closed competition' auctions. In both auction formats, suppliers place one-shot sealed bids. In open competitions, all interested bidders are allowed to place bids. In closed competitions, however, the procurer restricts the set of eligible bidders. Under special circumstances, procurers may deviate from these one-shot sealed bid auction formats and opt for direct purchase (no

[28]Given the possibility for procurers to tweak the format of government procurement auctions, the Norwegian Public Procurement Act takes steps to limit the possibility of cronyism. In particular, all procurers must recuse themselves if they have ties (for example, economic or social) with potential suppliers that prevent them from making an unbiased ranking of the incoming bids.

auction) or auction formats with multi-round negotition. In the latter auction format, suppliers place initial bids from which the procurer selects one or several bids for further negotiation. Multi-round negotiation may be used whenever the procurement's "character, complexity, legal, or financial composition or associated risk necessitates negotiation."

Under the Norwegian Public Procurement Act, procurers are allowed to impose requirements and criteria related to the chosen supplier's production process to ensure that government contracts are implemented in a manner that promotes respect for the environment, innovation, employment, and social conditions. For example, state, county, and municipal governments may include clauses in their procurement contracts that mandate wages and working conditions that are no worse than those provided by current nationwide collective agreements for the relevant sectors. A reform in 2008 particularly increased the minimum wage and work condition requirements for construction firms catering to government agencies.

The Norwegian Public Procurement Complaints Board (KOFA) exists to ensure that government procurers adhere to the Public Procurement Act and rank suppliers fairly according to the auction format. KOFA is an independent complaints body that handles disputes between government procurers and their suppliers, concerning breaches of Norwegian procurement regulations. Any supplier who has submitted a bid in a procurement auction may file a complaint to KOFA (for example, a supplier might feel the procurer has used his discretion over the procurement process to unfairly favor another supplier). While KOFA is not authorized to handle questions related to other competition law, KOFA can overrule any discretion of the procurer that is in conflict with the fundamental principles of the Public Procurement Act. The financial bar for filing complaints is low, with a filing fee of less than $1,000.[29]

[29]The KOFA appeals process creates a potential source of measurement error in our setting. Specifically, our procurement-level data, described in Section 3.1, are based on the Norwegian Public Roads Administration's (Public Roads) initial ranking of bidders, and do not account for subsequent appeals. Based on their own analyses, Public Roads officials inform us that their initial rankings are overturned in less than 1% of cases. Using data from KOFA, we find that only 106 Public Roads procurements have been brought before KOFA in the period 2003–2015. Of these 106 cases, Public Roads lost 28. All Public Roads procurements must be approved by an internal quality assurance committee before the final decision is made, which may explain why so few Public Roads cases are brought before KOFA.

After a supplier has been chosen and a procurement contract has been signed, situations might arise that call for changes to the initial contract. For example, the procurer's needs may have changed over time, or the supplier might wish to increase the quantity or quality of products delivered, or the procurer may wish to extend the duration of the contract. Whether or not renegotiation is allowed, depends on the value of the procurement. In general, however, the procurer is allowed to renegotiation certain non-substantial terms of the contract without conducting a new procurement auction (according to the legislation, whether or not a change to the contract is substantial depends on the specific context). The scope for renegotiation is somewhat greater for procurements below the EEA threshold values.

B.2 Procurement by the Norwegian Public Roads Administration

In this section, we provide more details on the procurement process in the Norwegian Public Roads Administration (Public Roads). To do so, we use data on all Public Roads procurement auctions in the period 2003–2015, described in more detail in Section 3. While in the main text, we restrict attention to a subset of the Public Roads auctions (specifically, we only consider price-only auctions), in the current appendix, we provide summary statistics from the full sample of Public Roads auctions without placing any restrictions on the data sample.

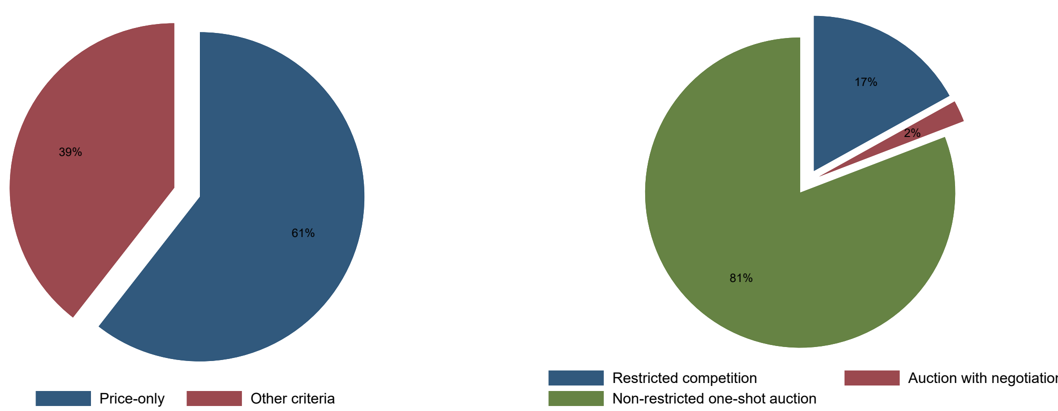
First, we describe the winning criteria and auction formats used by Public Roads. On winning criteria, Panel A of Figure A.II shows that 61% of all Public Roads auctions are determined using price as the only criterion while in 39% of the auctions, multiple criteria are used to determine the winners. On auction formats, Panel B of Figure A.II shows that 17% of the auctions are one-shot sealed bid where the Public Roads bureaucrat has limited competition by restricting the set of eligible bidders, while 81% of the auctions are one-shot sealed bid without any restrictions to competition. The remaining 2% of auctions are multi-round negotiation.[30]

[30] Note that auctions with multi-round negotiation can be restricted and non-restricted as well. For ease of exposition, we report "Auction with negotiation" as a separate group.

Second, we describe the product types Public Roads procure. In Panel A of Figure A.III, we provide a broad classification of the procured products. In 62% of the auctions, Public Roads procure construction while they procure services and materials in 30% and 6% of the auctions, respectively. In unreported results, we find that the fractions of overall value of procurement (as opposed to fractions of number of auctions) are 70%, 29%, and 1% for construction, services, and materials, respectively. Using a finer classification, Panel B of Figure A.III shows that 19% of all auctions are for road work while traffic safety measures are in second place with 5% of all auctions. In other top-ten products, we find consulting services, building of fences, demolition work, and smaller stretches of walking and cycling paths.

Figure A.II: Summary statistics: Winning criteria and auction format

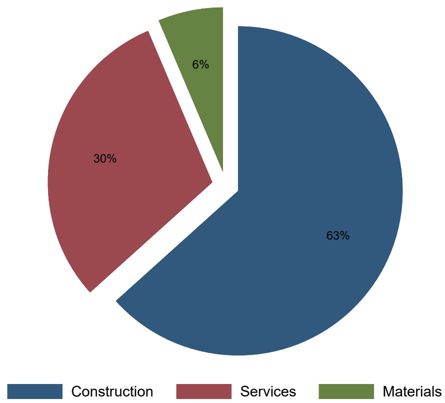
(a) Winning criteria (b) Auction format and market restriction



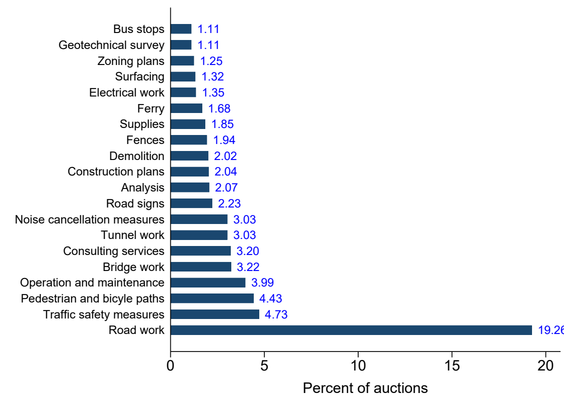
Note: The figure presents summary statistics from the full sample of Public Roads procurement auctions, described in more detail in Section 3. The sample comprises approximately 10,500 unique procurement auctions. In Panel A, we plot the distribution of winning criteria, where we have split between price-only auctions and multiple-criteria auctions. In Panel B, we plot the distribution of auction formats used, where we have split between non-restricted one-shot auctions, one-shot auctions with restricted competition, and multi-round auctions with negotiation. Note that "Auctions with negotiation" can either be restricted or non-restricted competition.

Figure A.III: Summary statistics: Public Roads products procured

(a) Broad classification



(b) Finer classification

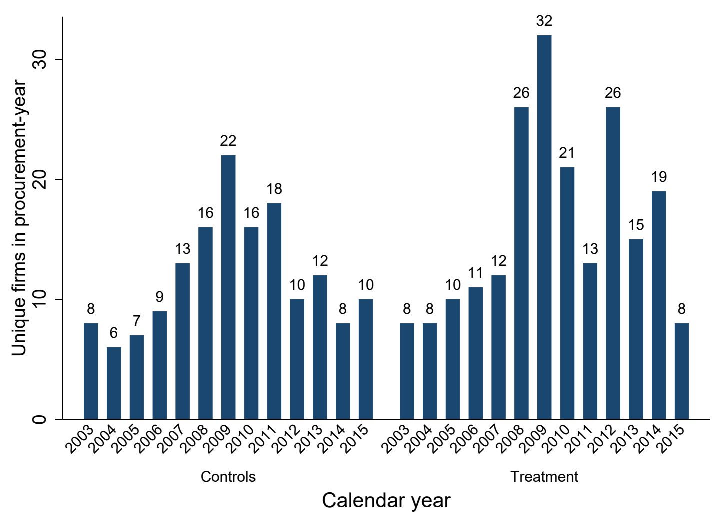


Note: The figure presents summary statistics from the full sample of Public Roads procurement auctions, described in more detail in Section 3. The sample comprises approximately 10,500 unique procurement auctions. In Panel A, we plot the distribution of a broad classification of products procured, where we have split between construction work, services, and material purchases. In Panel B, we plot the distribution of a finer classification of products procured, where we present the distribution of the 15 most-procured products. In both panels, fractions are computed as the count of auctions in a given category relative to the total number of auctions.

C Summary statistics: Startups by procurement-year

In Section 5, we estimate the long-run effects on startup outcomes of winning a government procurement auction. The estimation sample in Section 5 includes startups that win (treatment group) or come second (control group) in procurement auctions in the period 2003-2015. For firms that win multiple auctions, we only use the first auction win. In Figure A.IV, we count the number of treatment and control startups in the estimation sample by the calendar year in which they enter the sample (by winning or losing an auction). The figure shows that the overall count of treatment and control startups is highest during the recession years 2007–2009. However, the relative size of the treatment and control groups remains fairly constant across procurement years. Finally, note that several startups enter the estimation sample late. For these firms, we cannot measure long-run outcomes since our firm-level data ends in 2016.

Figure A.IV: Startups by procurement-year



Note: The figure plots the count of winners (treatment group) and runners up (control group) in our estimation sample by the calendar year in which they entered the estimation sample (the firm's 'procurement-year'). Observe that, for the treatment group, firms are unique both within-procurement-year and across-procurement-year because we only keep firms' first auction win. For the control group, in contrast, firms are unique within-procurement-year but may not be unique across-procurement-year since we allow non-winners to enter the control group in multiple years. Also note that firms may not be unique across the treatment-control divide. For example, a firm can, by bidding and losing, be counted in the control group in 2004 but also, by bidding and winning, be counted in the treatment group in 2007. Once treated, the firm cannot cross back to the control group.

D Placebo test: Runner-up vs. 3rd bid

In Tables 3 and Table 4 in the main text, we estimate the effect of winning a procurement auction on startup outcomes by comparing the change in outcomes for winning firms with the change in outcomes for runners up. As a placebo test, in the current appendix, we estimate the effect of being a runner-up relative to being ranked third. To do so, we use the same procedure to build the estimation sample and the same regression model as described in Section 4.1 and 4.2.[31] However, the 'treatment' group now comprises runners up while the 'control' group comprises third-placed firms. Since neither of these firms win auctions, we should not see any effects on outcomes. Indeed, Table A.I shows that there is no statistically or economically significant effect on key firm outcomes of being a runner-up compared to being ranked third.

Table A.I: Placebo test: Runner-up vs. 3rd bid

	Firm outcomes						
	Log(Sales)	Log(Value added)	Log(Empl.)	Active	Log(Capital)	Log(TFP)	Competes
τ (Contract)	-0.02	-0.01	-0.04	-0.01	0.07	-0.05	0.01
	(-0.26)	(-0.11)	(-0.52)	(-0.29)	(0.48)	(-1.01)	(0.21)
τ (Post-contract)	0.09	0.11	0.01	0.01	0.09	-0.05	0.01
	(0.90)	(1.03)	(0.18)	(0.17)	(0.63)	(-0.95)	(0.24)
Adj R^2	0.78	0.79	0.84	0.36	0.77	0.60	0.51
N	2104	2086	2015	2593	1988	1863	2593

Note: The table reports estimates of θ from the following regression: $y_{jek} = \theta b_{jek} + \kappa_t + \alpha_e + \lambda_{jk} + \varepsilon_{jek}$, where y_{jek} is the outcome for firm j in event-time e centered on auction k; b_{jek} equals one for auction winners in the post-auction period, zero otherwise; and κ_t, α_e, and λ_{jk} are calendar-time, event-time, and firm-in-auction fixed effects, respectively. The coefficient θ is allowed to differ in the procurement contract-period and post-contract period. The outcomes are Log(Sales); Log(Value added); Log(Employees); a dummy for whether the firm is active; Log(Capital); Log(TFP); and a dummy for whether the firm competes in any procurement auctions. The estimation sample holds runner-up and third-placed bidders. Runners up are defined as the 'treatment' group and third-placed bidders are defined as the 'control' group. Standard errors are clustered at the firm-level. t-statistics in parentheses. Stars (***, **, *) indicate statistical significance at the 10, 5, and 1 percent level.

[31] As we focus on runner-up and third-placed bids, we restrict attention to auctions with at least three bidders.

E Are startups unique?

In Section 5 of the main text, we explore the effects of procurement auction wins on startup sales and employment. We find that auction wins have large, positive, and persistent effects on startups' sales and employment. In the current appendix, we estimate the effects of auction wins for mature firms. First, in Section E.1, we describe differences and similarities between startups and mature firms. In Section E.2, we use a propensity score matching approach to identify mature firms that have similar characteristics as startups. Finally, in Section E.3, we explore the effects of auction wins on startups, mature firms, and mature firms with similar characteristics as startups. We do not find effects of auction wins for mature firms.

E.1 Summary statistics: Startups and mature firms

In Panel A of Table A.II, we present differences-in-means between the sampled startups and mature firms in the year before a focal auction (as defined in Section 4.1). Since in Section E.2 we are only interested in finding mature winners with startup characteristics, the sample in Table A.II compares startups (winners and losers) and mature winners. Panel A of Table A.II shows that mature winners on average are widely different from startups. For example, their average sales are NOK 334 million larger, they have 114 more employees, and earn NOK 11 million more in profits. These differences are highly significant, both economically and statistically.

E.2 Matching startups and mature firms

Panel A of Table A.II shows that startups and mature firms on average are very different in key characteristics such as employment, sales and assets. While startups and mature firms differ on average, some of the sampled mature firms could have characteristics that are similar to that of startups. To identify such 'pseudo-startups', we use a propensity score matching approach. Using the same sample as in Section E.1, we match the startup and mature firm samples on the number of employees, sales, and capital stock. We allow for up to two matches for each startup and a matching radius (caliper) of 0.1. The matching is done separately for each procurement year, using firm characteristics from the year before the procurement.

Panel B of Table A.II presents differences-in-means between the sample of startups and their matched 'pseudo-startups'. While some differences remain between the two groups, the startups and their matched mature counterparts appear fairly comparable. For example, we do not find statistically significant differences in the number of employees, total assets, or dividends between the startup and 'pseudo-startup' samples.

Table A.II: Comparing sampled startups to mature firms

	Panel A: Before matching				
	Sales	Employees	Total assets	Dividends	Profits
Startup	-334.49**	-114.70***	-262.24*	-2.80***	-11.46*
	(-2.21)	(-4.50)	(-1.89)	(-3.14)	(-1.83)
Adj R^2	-0.00	0.02	-0.00	0.01	-0.00
N	840	809	840	840	840
	Panel B: After matching				
	Sales	Employees	Total assets	Dividends	Profits
Startup	-7.84**	-1.45	-7.04	-0.64	-0.62**
	(-2.24)	(-0.41)	(-1.44)	(-1.48)	(-2.15)
Adj R^2	0.00	0.00	0.00	-0.01	0.03
N	545	522	545	545	545

Note: Panel A reports the difference-in-means between sampled startups and mature winners in the year before their focal auction (as defined in Section 4.1). Panel B reports the difference-in-means between sampled startups and their matched 'pseudo-startups'. To identify 'pseudo-startups', we match (using propensity score matching) the startup and mature firm samples on number of employees, sales, and capital stock. We allow up to two matches for each startup and a matching radius (caliper) of 0.1. The matching is done separately for each procurement year using firm characteristics from the year before the focal procurement auction.

E.3 Effects of auction wins for mature firms

Having described the differences and similarities between startups and mature firms, we turn to estimating the effects of procurement auction wins for startups, mature firms, and mature firms with the characteristics of startups ('pseudo-startups'). To do so, we use the same empirical methodology as in the main analysis, described in Section 4. The results are presented in Table A.III. In columns labeled 'Baseline effect', we estimate the effects of auction wins separately for startups, mature firms, and 'pseudo-startups'. For startup winners, we use startup losers as control firms. For mature and 'pseudo-startup' winners, we use mature firms as control firms. In columns labeled 'High treat intensity', we restrict the sample of winners to those with high treatment intensity. In columns labeled 'Low treat intensity', we restrict the sample of winners to those with low treatment intensity. Treatment intensity is measured as total auction winnings divided by firm sales in event time -1. Low treatment corresponds to quintiles 1–3 of treatment intensity, while high treatment corresponds to quintiles 4–5. Overall, we find large and persistent effects of auction wins for startups, but not for mature firms.

Table A.III: Are startups unique?

Panel A: Log(Value added)

	Baseline effect			High treat intensity			Low treat intensity		
	Startups	Mature	Matched	Startups	Mature	Matched	Startups	Mature	Matched
θ (Contract)	0.19**	0.02	-0.01	0.34***	-0.03	-0.05	0.09	0.04	0.01
	(2.11)	(0.34)	(-0.35)	(2.81)	(-0.34)	(-0.88)	(0.92)	(1.15)	(0.25)
θ (Post-contract)	0.19*	0.02	-0.03	0.42***	-0.04	-0.16*	0.09	0.05	0.09
	(1.95)	(0.43)	(-0.55)	(3.00)	(-0.54)	(-1.83)	(0.86)	(1.20)	(1.52)
Adj R^2	0.77	0.90	0.89	0.76	0.88	0.89	0.77	0.91	0.90
N	2926	8886	6059	1833	5811	4939	2343	6967	5012

Panel B: Log(Employees)

	Baseline effect			High treat intensity			Low treat intensity		
	Startups	Mature	Matched	Startups	Mature	Matched	Startups	Mature	Matched
θ (Contract)	0.14*	0.04	0.03	0.35***	0.04	0.04	0.04	0.04	0.02
	(1.95)	(1.15)	(0.87)	(3.03)	(0.64)	(0.85)	(0.55)	(1.39)	(0.44)
θ (Post-contract)	0.22***	0.04	0.04	0.54***	0.06	0.00	0.10	0.02	0.06
	(2.99)	(1.12)	(0.82)	(4.05)	(1.05)	(0.07)	(1.26)	(0.71)	(1.23)
Adj R^2	0.82	0.93	0.92	0.82	0.92	0.92	0.83	0.93	0.93
N	2776	8725	5938	1737	5651	4817	2246	6887	4934

Note: The table reports estimates of θ from the following regression: $y_{jek} = \theta b_{jek} + \kappa_t + \alpha_e + \lambda_{jk} + \varepsilon_{jek}$, where y_{jek} is the outcome for firm j in event-time e centered on auction k; b_{jek} equals one for auction winners in the post-auction period, zero otherwise; and κ_t, α_e, and λ_{jk} are calendar-time, event-time, and firm-in-auction fixed effects, respectively. The coefficient θ is allowed to differ in the procurement contract-period and post-contract period, and is estimated separately for startups, mature firms and matched firms. In columns labeled "Matched firms", the treatment group comprises mature firms that have the characteristics of startups, as identified using a propensity score matching approach (described in Appendix E.2), while the control group comprises mature runners up. In columns labeled "Baseline effect", we report θ using the full sample of either startups or mature firms. In columns labeled "High treatment intensity", we restrict attention to firms with high treatment intensity. In columns labeled "Low treatment intensity, we restrict attention to firms with low treatment intensity. Treatment intensity is measured as total auction winnings divided by firm sales in event time -1. Low treatment corresponds to quintiles 1–3 of treatment intensity, while high treatment corresponds to quintiles 4–5. In Panel A, the outcome is Log(Value added). In Panel B, the outcome is Log(Employees). Standard errors are clustered at the firm-level. t-statistics in parentheses. Stars (***, **, *) indicate statistical significance at the 10, 5, and 1 percent level.

F Robustness: Alternative estimation samples

In the main text, we estimate the effects of procurement wins on startup outcomes. As explained in Section 4.1, as our treatment group, we use startups that have not won an auction before. As the control group, we use firms that are runners up and have not won an auction before. We allow a control group firm to enter the sample multiple times if it comes second in more than one auction. In practice, to allow control firms to enter the sample multiple times, we include a separate copy of the firm's time series surrounding each of the auction participation years.

In Table A.IV, we explore robustness to two alternative sample specifications. First, in Panel A, we follow Cellini et al. (2010) and include all auctions in the sample and estimate the average effect across all auctions. Compared to the main sample, where we only use firms' first win, the Cellini et al. (2010) approach implies also including winners' subsequent auctions (with separate time-series surrounding each of the subsequent auction participation years). With this sample, we obtain stronger effects than with the preferred estimation sample, Table A.IV shows. Second, in Panel B of Table A.IV, we take a step back and only include firms' first auction in the sample. Compared to the main sample, this implies that we do not allow control firms to enter the sample multiple times (firms are represented in the sample exactly once). The results in Panel B of Table A.IV are almost identical to the main results.

Table A.IV: Robustness: Alternative estimation samples

	Log(Sales)	Log(Value added)	Log(Empl.)	Active	Log(Capital)	Log(TFP)	Competes
	Panel A: Using all auctions						
	Log(Sales)	Log(Value added)	Log(Empl.)	Active	Log(Capital)	Log(TFP)	Competes
θ (Contract)	0.28***	0.19**	0.15**	0.00	0.16*	0.00	0.03
	(3.37)	(2.51)	(2.56)	(0.06)	(1.77)	(0.06)	(1.23)
θ (Post-contract)	0.27***	0.22**	0.20***	0.02	0.39***	-0.07	0.14***
	(2.88)	(2.59)	(2.88)	(0.54)	(3.11)	(-1.60)	(4.22)
Adj R^2	0.77	0.78	0.82	0.36	0.75	0.59	0.51
N	4745	4698	4479	5671	4507	4207	5671
	Panel B: Using only first auction						
	Log(Sales)	Log(Value added)	Log(Empl.)	Active	Log(Capital)	Log(TFP)	Competes
θ (Contract)	0.23**	0.17*	0.11	-0.01	0.17	0.04	0.04
	(2.27)	(1.66)	(1.33)	(-0.21)	(1.33)	(0.76)	(1.42)
θ (Post-contract)	0.26**	0.20*	0.20**	-0.03	0.43***	-0.06	0.13***
	(2.23)	(1.76)	(2.30)	(-0.48)	(2.66)	(-1.11)	(3.15)
Adj R^2	0.76	0.76	0.83	0.36	0.74	0.60	0.56
N	2540	2524	2384	3178	2394	2256	3178

Note: The table reports estimates of θ from the following regression: $y_{jek} = \theta b_{jek} + \kappa_t + \alpha_e + \lambda_{jk} + \varepsilon_{jek}$, where y_{jek} is the outcome for firm j in event-time e centered on auction k; b_{jek} equals one for auction winners in the post-auction period, zero otherwise; and κ_t, α_e, and λ_{jk} are calendar-time, event-time, and firm-in-auction fixed effects, respectively. The coefficient θ is allowed to differ in the procurement contract-period and post-contract period. In Panel A, we follow Cellini et al. (2010) and use all auctions. In Panel B, we only use a firm's first auction participation. The outcomes are Log(Sales); Log(Value added); Log(Employees); a dummy for whether the firm is active; Log(Tangible assets); Log(TFP); and a dummy for whether the firm competes in other procurement auctions.. Standard errors are clustered at the firm-level. t-statistics in parentheses. Stars (***, **, *) indicate statistical significance at the 10, 5, and 1 percent level.

G Robustness: Auctions with at least 3 bidders

In the main text, we compare procurement auction winners to runners up in a difference-in-differences design. We do not distinguish between auctions that are likely to be more or less competitive. In the current appendix, we re-estimate our main results using the sub-sample of firms that compete in auctions with three or more bidders. The idea behind this test is that increasing the number of bidders should increase the competitiveness, thus making it more likely that auction outcomes are randomly assigned between winners and runners up. The results are presented in Table A.V. The estimates in Table A.V are qualitatively similar to the main results. However, the statistical precision is somewhat lower.

Table A.V: Robustness: Auctions with at least 3 bidders

	Firm outcomes						
	Log(Sales)	Log(Value added)	Log(Empl.)	Active	Log(Capital)	Log(TFP)	Competes
θ (Contract)	0.27***	0.17*	0.17**	0.00	0.13	0.05	0.04
	(2.92)	(1.77)	(2.03)	(0.10)	(0.97)	(0.91)	(1.51)
θ (Post-contract)	0.19*	0.14	0.20**	0.01	0.34**	-0.04	0.14***
	(1.84)	(1.31)	(2.47)	(0.12)	(2.09)	(-0.78)	(3.23)
Adj R^2	0.77	0.77	0.82	0.37	0.73	0.62	0.53
N	2235	2221	2115	2804	2135	2009	2804

Note: The table reports estimates of θ from the following regression: $y_{jek} = \theta b_{jek} + \kappa_t + \alpha_e + \lambda_{jk} + \varepsilon_{jek}$, where y_{jek} is the outcome for firm j in event-time e centered on auction k; b_{jek} equals one for auction winners in the post-auction period, zero otherwise; and κ_t, α_e, and λ_{jk} are calendar-time, event-time, and firm-in-auction fixed effects, respectively. The coefficient θ is allowed to differ in the procurement contract-period and post-contract period. The outcomes are Log(Sales); Log(Value added); Log(Employees); profits; a dummy for dividende payments; and a dummy for whether the firm is active. Standard errors are clustered at the firm-level. t-statistics in parentheses. Stars (***, **, *) indicate statistical significance at the 10, 5, and 1 percent level.

H Robustness: Auctions with closer win margin

In the main text, we compare procurement auction winners to runners up in a difference-in-differences design. We do not distinguish between auctions that are likely to be more or less competitive. In the current appendix, we re-estimate our main results using the sub-sample of firms that compete in auctions where the win margin is smaller than 10%. The results are presented in Table A.VI. The estimates in Table A.VI are qualitatively similar to the main results. However, statistical precision is somewhat lower. Using even smaller win margins, for example 5%, leads to qualitatively similar results but even less precision.

Table A.VI: Robustness: Auctions with closer win margin

	Firm outcomes						
	Log(Sales)	Log(Value added)	Log(Empl.)	Active	Log(Capital)	Log(TFP)	Competes
θ (Contract)	0.27**	0.13	0.18**	0.06	0.07	0.00	0.04
	(2.39)	(1.09)	(2.20)	(1.01)	(0.43)	(0.06)	(1.21)
θ (Post-contract)	0.24*	0.19	0.25**	0.05	0.30	-0.01	0.13**
	(1.78)	(1.43)	(2.37)	(0.69)	(1.46)	(-0.13)	(2.57)
Adj R^2	0.74	0.75	0.79	0.37	0.76	0.65	0.52
N	1526	1515	1444	1881	1441	1345	1881

Note: The table reports estimates of θ from the following regression: $y_{jek} = \theta b_{jek} + \kappa_t + \alpha_e + \lambda_{jk} + \varepsilon_{jek}$, where y_{jek} is the outcome for firm j in event-time e centered on auction k; b_{jek} equals one for auction winners in the post-auction period, zero otherwise; and κ_t, α_e, and λ_{jk} are calendar-time, event-time, and firm-in-auction fixed effects, respectively. The coefficient θ is allowed to differ in the procurement contract-period and post-contract period. The outcomes are Log(Sales); Log(Value added); Log(Employees); profits; a dummy for dividende payments; and a dummy for whether the firm is active. Standard errors are clustered at the firm-level. t-statistics in parentheses. Stars (***, **, *) indicate statistical significance at the 10, 5, and 1 percent level.

I External validity: Sampling weights

The current appendix has two subsection. In Appendix I.1, we provide summary statistics of the industry composition of sampled startups and compare it to the industry composition of the population of Norwegian startups. We find that construction firms are over-represented in the sample. In Appendix I.2, we re-weight our main regressions from Section 5 to match the population industry composition and find qualitatively similar results.

I.1 Population and sample industry weights

In Panel A of Figure A.V, we summarize the industry composition in our estimation sample using sample industry weights. The sample industry weights are constructed by counting the number of firms in a given industry and dividing this count by the overall number of firms in our estimation sample (the estimation sample is described in Section 4.1). As expected, the construction industry is heavily represented in our sample — 70% of the firms in our sample are construction firms. Among other represented industries, we find "Other business activities" at 11%, "Retail" at 5%, and "Transport and telecom" at 4%.

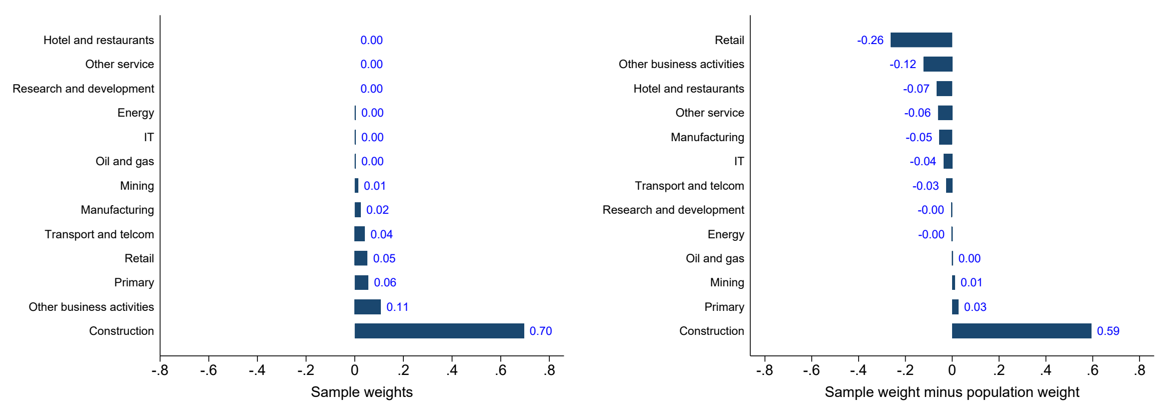
In Panel B of Figure A.V, we compare the sample industry weights to the industry weights in the population of Norwegian firms. To construct population industry weights, we count the number of Norwegian firms in each industry and divide this count by the total number of Norwegian firms (for comparability with our estimation sample, we only use startups to compute the population industry weights). Compared to the population, construction firms are heavily over-represented in our sample, while primary industries are only slightly over-represented. By contrast, retail firms and other business activities are heavily under-represented in our sample, carrying sample weights that are 26% and 12% smaller than in the population.

I.2 Regressions with sampling weights

In this section, we re-estimate our main regressions, described in Section 4, using sampling weights. The sampling weights are constructed by dividing the population industry weight by the sample industry weights. In practice, this weighting scheme gives construction firms, the dominant industry in our sample, a much smaller weight in the regressions, while real estate and retail firms receive much higher weights (see Figure A.V). The estimated coefficients, presented in Table A.VII, are qualitatively similar to the main results in Section 5. However, the coefficients are now estimated with less statistical precision.

Figure A.V: Sample and population industry weights

(a) Sample weights (b) Sample weight – population weight



Note: In Panel A, we plot the estimation sample industry weights. As explained in Section 4.2., the estimation sample only comprises startups surrounding their first auction win. In Panel B, we plot the difference between the sample industry weights and the industry weights in the population of Norwegian startups. The population of startups are defined analogously to our sampled startups — i.e., they are less than 10 years old, are not a subsidiary, and have less than NOK 16 million in assets in both their first and second year of operation.

Table A.VII: Robustness: Regressions with sampling weights

	Firm outcomes						
	Log(Sales)	Log(Value added)	Log(Empl.)	Active	Log(Capital)	Log(TFP)	Competes
θ (Contract)	0.21	0.19	0.06	0.03	0.18	0.00	0.00
	(1.15)	(1.13)	(0.45)	(0.43)	(0.61)	(0.05)	(0.00)
θ (Post-contract)	0.37*	0.27	0.30**	-0.01	0.46	-0.00	-0.01
	(1.93)	(1.44)	(2.46)	(-0.09)	(1.26)	(-0.03)	(-0.16)
Adj R^2	0.76	0.76	0.86	0.30	0.79	0.70	0.55
N	2767	2752	2619	3373	2618	2482	3373

Note: The table reports estimates of θ from the following regression: $y_{jek} = \theta b_{jek} + \kappa_t + \alpha_e + \lambda_{jk} + \varepsilon_{jek}$, where y_{jek} is the outcome for firm j in event-time e centered on auction k; b_{jek} equals one for auction winners in the post-auction period, zero otherwise; and κ_t, α_e, and λ_{jk} are calendar-time, event-time, and firm-in-auction fixed effects, respectively. The coefficient θ is allowed to differ in the procurement contract-period and post-contract period. The outcomes are Log(Sales); Log(Value added); Log(Employees); profits; a dummy for dividende payments; and a dummy for whether the firm is active. Standard errors are clustered at the firm-level. t-statistics in parentheses. Stars (***, **, *) indicate statistical significance at the 10, 5, and 1 percent level.

J Robustness: Alternative TFP measures

In the current appendix, we provide additional details on how we construct our measures of total factor productivity (TFP). In Section J.1, we provide a basic overview of TFP measurement and construct three separate TFP measures. In Section J.2, we report the effects of procurement auction wins on our three TFP measures. In Section J.3, we explore whether procurement auction wins impact firms' production technologies.

J.1 Measuring TFP

The starting point for estimating firm-level TFP is a production function representing how a firm's inputs are combined to produce output. Suppose, for example, that output Y_{it} for firm i in calendar year t is produced using two input factors, physical capital K_{it} and labor capital L_{it}, using a Cobb-Douglas production function:

$$Y_{it} = A_{it} \cdot K_{it}^{\beta_k} \cdot L_{it}^{\beta_l}. \tag{A.I}$$

In this specification, the exponents β_k and β_l represent the output elasticities of physical and labor capital, respectively, while the parameter A_{it} can be interpreted as a measure of TFP, capturing variations in output Y_{it} that cannot be not explained by shifts in the inputs K_{it} and L_{it}. Whereas Y_{it}, K_{it} and L_{it} are typically all observable, A_{it} is unobservable and must be estimated. The purpose of this appendix is to provide consistent estimates of A_{it}.

As a starting point, we estimate A_{it} using ordinary least squares. To facilitate linear estimation of A_{it}, we transform the production function in equation A.I with natural logarithms. Using small letters for logged variables, the estimation equation becomes:

$$y_{it} = \beta_0 + \beta_k k_{it} + \beta_l l_{it} + \epsilon_{it}, \tag{A.II}$$

where

$$\ln(A) = \beta_0 + \epsilon_{it}. \tag{A.III}$$

Thus, TFP is defined by two components: β_0, which captures the mean efficiency across firms and ϵ_{it}, which captures time and firm-specific deviations from that mean. This basic representation of TFP is widely used in the existing productivity literature (e.g., Syverson 2011). Estimating equation A.II using OLS and solving for TFP, we get:

$$\hat{\omega}_{it}^{OLS} = \hat{\beta}_0 + \hat{\epsilon}_{it} = y_{it} - \hat{\beta}_k k_{it} - \hat{\beta}_l l_{it}. \tag{A.IV}$$

To estimate $\hat{\omega}_{it}^{OLS}$, we use firm-year observations on all our sampled firms in the time period 1992–2016 (see Section 3 for more details on sample selection). As empirical proxies for y_{it}, k_{it} and l_{it}, we use value added, capital stock and total wage bill, respectively. To accommodate that startups and mature firms might have different production technologies, we estimate $\hat{\omega}_{it}^{OLS}$ separately for the sampled startups and mature firms.

Columns 1 and 2 of Table A.VIII present the estimates of $\hat{\omega}_{it}^{OLS}$ for startups and mature firms, respectively. Three key observations emerge from Table A.VIII. First, $\beta^l >> \beta^k$ for both startups and mature firms, with β^l being 0.12 for young firms and 0.11 for mature firms, and β^k being 0.77 and 0.82 for statups and mature firms respectively. Second, both startups and mature firms in our sample exhibit decreasing returns to scale, as shown by $\beta^k + \beta^l < 1$. Third, the sum $\beta^k + \beta^l$ is consistently smaller for startups than mature firms.

Meanwhile, a large literature on TFP measurement has identified problems with OLS estimates of TFP (see Syverson 2011 for a survey). One concern is identification. OLS requires that the inputs in the production function are exogenous or, equivalently, determined independently from firms' TFP. This requirement may not be satisfied if firms choose inputs based on their own productivity or if we have omitted from equation A.I input factors that are correlated

Table A.VIII: Estimates of output elasticities

	OLS estimates		LP estimates	
	Young	Old	Young	Old
β^k	0.12***	0.11***	0.10***	0.09***
	(15.15)	(13.59)	(7.40)	(8.87)
β^l	0.77***	0.82***	0.67***	0.71***
	(56.85)	(53.91)	(29.30)	(25.62)
$\beta^k + \beta^l$	0.897	0.925	0.772	0.807
N	9784	11512	9409	11171

Note: t-statistics in parentheses. Stars (***, **, *) indicate statistical significance at the 10, 5, and 1 percent level. The production functions are estimated separately for young firms and old firms using data from 1992–2016. We provide estimates of β^k and β^l from OLS and Levinsohn-Petrin.

with both output and included inputs. In such scenarios, OLS inconsistently estimates β_k and β_l, leading to inconsistent estimates of TFP. Another concern is production technology. With OLS, we put no structure on the output elasticities, meaning that (for example) an increase in output might mistakenly be attributed to scale economies rather than productivity increases.

To address these identification concerns, we identify output elasticities using the Levinsohn and Petrin (2003) approach. Extending Olley and Pakes (1996), Levinsohn and Petrin (2003) use a structural model of an optimizing firm to derive the conditions under which intermediate inputs can be used to proxy for unobserved productivity in the production function, thus providing consistent estimates of the output elasticities β_k and β_l. Columns three and four of Table A.VIII present the estimates of β_k and β_l from the Levinsohn-Petrin approach, estimated separately for startups and mature firms. The β_k coefficients are reasonably similar in both the OLS and Levinsohn-Petrin columns. In contrast, OLS significantly overstates β^l.

Acknowledging the many identification concerns described in the existing literature, we also follow Bloom et al. (2013) and calibrate equation A.II using output elasticities identified by existing research. The benefits of calibration is that we circumvent identification of β_k and β_l while still being able to impose a specific production function (in our case, constant returns to scale). The disadvantage, however, is that we must find output elasticities that are reasonable in the context of the Norwegian construction industry. Using the same output elastiticies as Bloom et al. (2013), we estimate TFP using the following model:

$$\hat{\omega}_{it}^{CRT} = y_{it} - 0.42k_{it} - 0.58l_{it} \qquad (\beta_k + \beta_l = 1) \tag{A.V}$$

Under the assumptions that the output elasticities are correctly specified and that production technologies indeed feature constant returns to scale ($\beta_k + \beta_l = 1$), equation A.V provides consistent estimates of TFP at the firm-year level.

J.2 Effects of auction wins on TFP

In the main text, we estimate the effects of procurement auction wins on TFP, measured using the Bloom et al. (2013) approach in equation A.V. In the current subsection, we re-estimate our main results using TFP measures based on OLS and the Levinsohn and Petrin (2003) approach. As in the main text, the empirical specification is a difference-in-differences model where we compare auction winners to runners up in years before and after the auction year. The estimation sample is defined in Section 3. The results, presented in Table A.IX, show that there is no long-run effect of auction wins on any of our three measures of TFP.

Table A.IX: Effects of auction wins on alternative TFP measures

	Measures of Log(TFP)		
	OLS	Levinsohn-Petrin	Bloom et al. (2014)
θ (Contract)	0.06	0.07*	0.04
	(1.33)	(1.66)	(0.87)
θ (Post-contract)	-0.04	-0.01	-0.05
	(-1.30)	(-0.43)	(-1.17)
Adj R^2	0.39	0.48	0.62
N	2709	2709	2627

Note: The table reports estimates of θ from the following regression: $y_{jek} = \theta b_{jek} + \kappa_t + \alpha_e + \lambda_{jk} + \varepsilon_{jek}$, where y_{jek} is the outcome for firm j in event-time e centered on auction k; b_{jek} equals one for auction winners in the post-auction period, zero otherwise; and κ_t, α_e, and λ_{jk} are calendar-time, event-time, and firm-in-auction fixed effects, respectively. The coefficient θ is allowed to differ in the procurement contract-period and post-contract period. Standard errors are clustered at the firm-level. t-statistics in parentheses. Stars (***, **, *) indicate statistical significance at the 10, 5, and 1 percent level.

J.3 Stability of output elasticities

Winning an auction could impact firms' production technology β_k and β_l. For example, winning firms might become more capital-intensive compared to losing firms. If production technologies change as a result of winning auctions, TFP should be measured using variable output elasticities rather than the fixed ones we use in Appendix J.2.

To test for the stability of output elasticities, we follow Akerman et al. (2015) and estimate the following difference-in-differences model:

$$y_{it} = \alpha_i + \alpha_t + \kappa \boldsymbol{x}_{it} + \beta D_{it} \boldsymbol{x}_{it} + \omega D_{it} + \varepsilon_{it}, \tag{A.VI}$$

where α_i and α_t are firm and time effects; $\boldsymbol{x}_{it}$ is a vector of factor inputs; and D_{it} is a dummy for treatment. The coefficients of interest are the interaction terms between the treatment indicator and the factor inputs, which capture the change in factor output elasticities due to treatment. We estimate equation (A.VI) separately for startups and mature firms using the same estimation sample as in Appendix J.2. The estimates are presented in Table A.X. The table shows that winning treatment auctions has no impact on the output elasticities β_k and β_l.

Table A.X: Stability of output elasticities

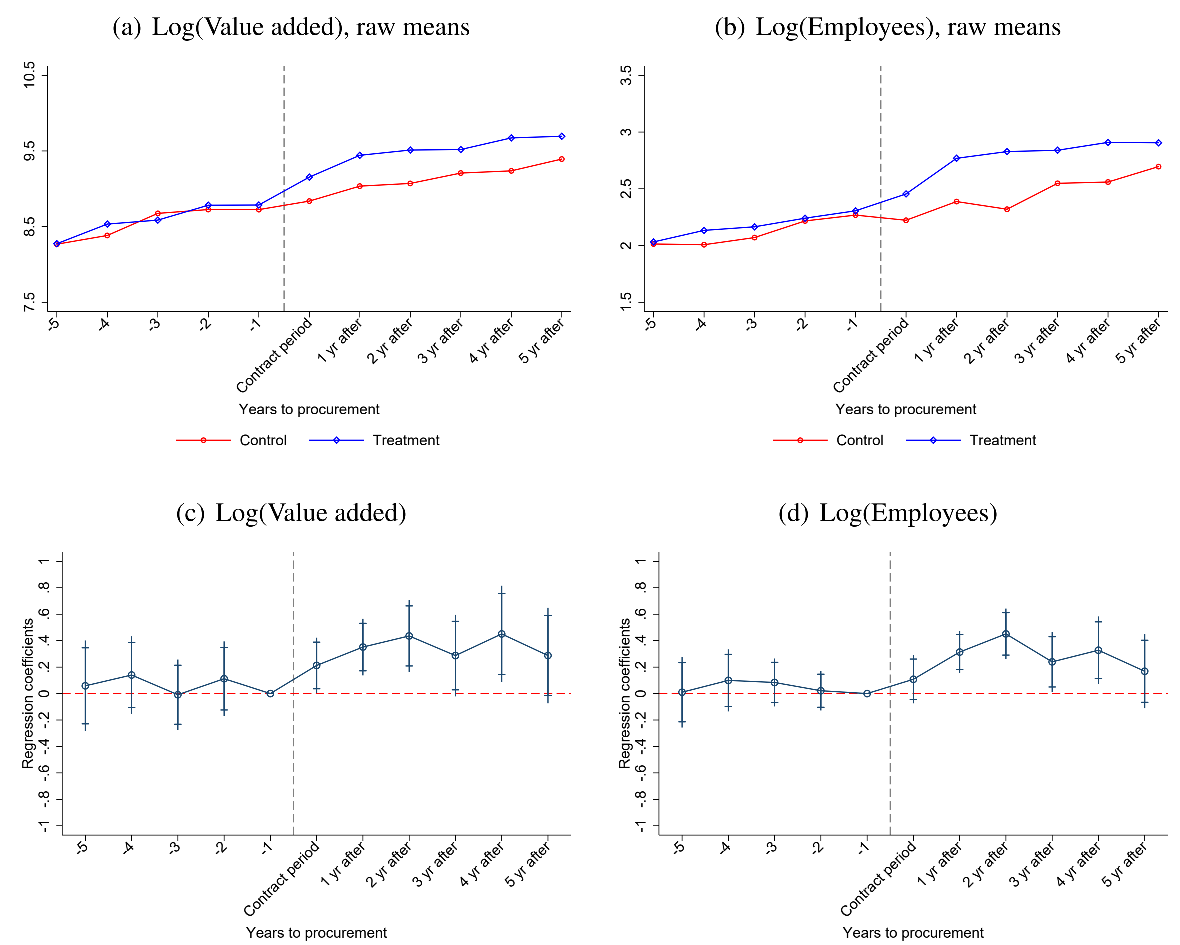
	Using all auctions		Using first-time winners	
	Young	Old	Young	Old
Win	0.04	0.08	-0.22	0.07
	(0.10)	(0.83)	(-0.44)	(0.43)
β^l	0.52***	0.80***	0.68***	0.72***
	(4.99)	(15.55)	(9.05)	(10.70)
β^k	0.07***	0.02**	0.07***	0.04***
	(2.82)	(2.33)	(2.64)	(3.58)
$\beta^l \times Win$	-0.01	-0.01	0.02	-0.01
	(-0.13)	(-0.85)	(0.45)	(-0.28)
$\beta^k \times Win$	0.00	0.00	0.00	-0.00
	(0.25)	(0.51)	(0.19)	(-0.09)
Adj R^2	0.91	0.98	0.92	0.96
N	4368	19622	2709	8535

Note: The table reports estimates from the following regression specification: $y_{it} = \alpha_i + \alpha_t + \kappa \boldsymbol{x}_{it} + \beta D_{it} \boldsymbol{x}_{it} + \omega D_{it} + \varepsilon_{it}$, where α_i and α_t are firm and time effects; $\boldsymbol{x}_{it}$ is a vector of factor inputs; and D_{it} is a dummy for treatment. The coefficient are the interaction terms between treatment and factor inputs, which capture the change in factor inputs due to treatment. The regression is estimated separately for two samples: in the first sample, we use all procurement auctions, while in the second sample, we only focus on first-time winners (this is the estimation sample from the main text, see Section 4.1). Standard errors are clustered at the firm-level. t-statistics in parentheses. Stars (***, **, *) indicate statistical significance at the 10, 5, and 1 percent level.

K Event study plots

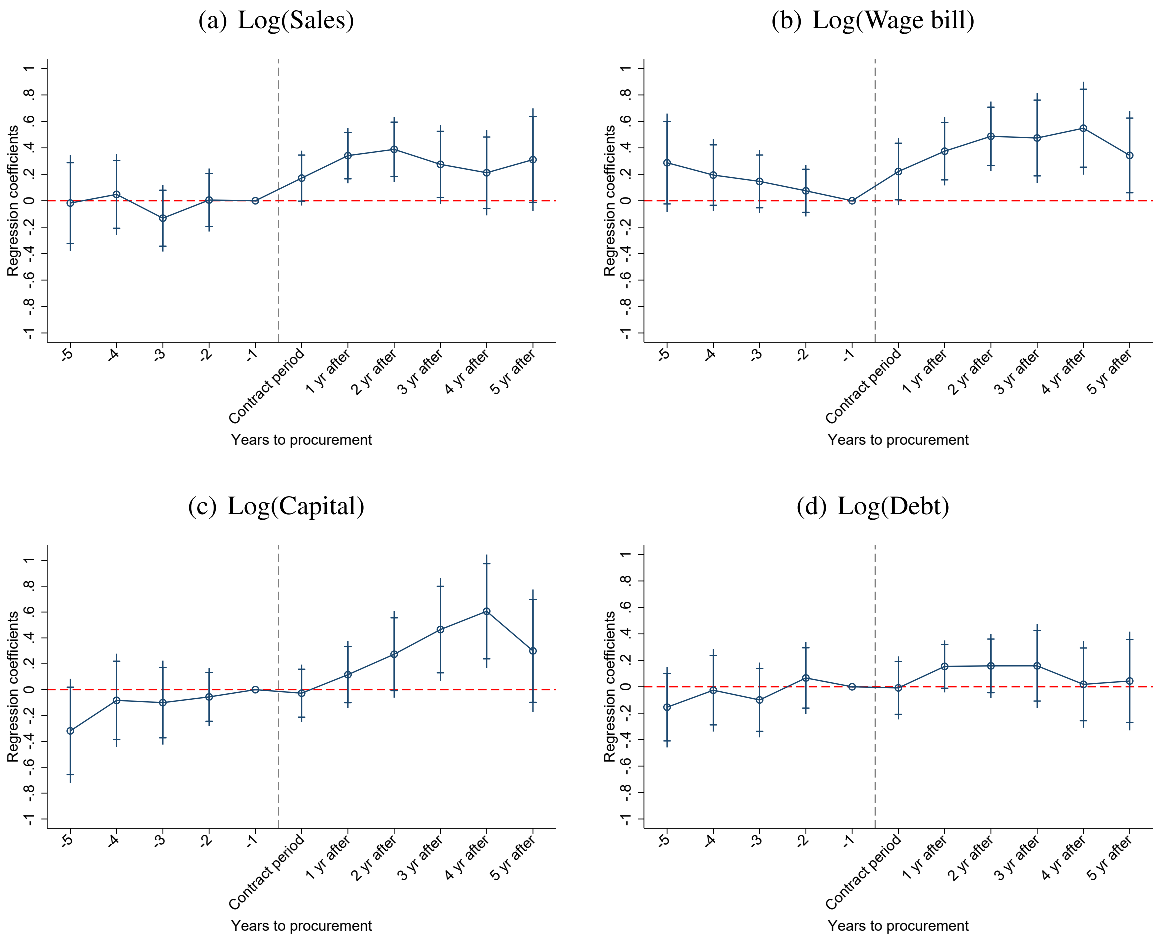
In the main text, we explore the effects of procurement auction wins by comparing auction winners to runners up in a difference-in-differences design. We show that auction winners are about 20% larger than runners up even several years after the procurement auction, despite being indistinguishable before the auction. In the current appendix, we present event study plots showing the evolution of firm characteristics both before and after procurement auctions. The event study plots present both raw means and the difference-in-difference coefficients between auction winners and runners up, with corresponding 95% confidence intervals.

Figure A.VI: Event study plots: Log(Value added) and Log(Employees)



Note: In Panels A–B, we plot raw means of Log(Value added) and Log(Employees) over event-time, separately for treatment (winners) and control (runners up) group firms. Event-time takes on negative values in years preceding the procurement auction and, in the post-auction period, event-time is measured relative to the procurement contract duration. Since treated and control firms can have different contract periods — for all control firms, we impute a contract period of 12 months — treated and control firms may not be balanced in terms of firm age in the post-contract period (this issue does not arise in the main text, because we do not measure event time relative to the contract period). For this reason, in Panels C–D, we plot the estimated difference-in-differences effect between winners and runners up over event-time, controlling for firm age fixed effects, with corresponding 90% (inner notches) and 95% (outer notches) confidence intervals. Standard errors are clustered at the firm level.

Figure A.VII: Additional outcomes



Note: The figure plots the estimated difference-in-differences effect between winners and runners up over event-time, with corresponding 90% (inner notches) and 95% (outer notches) confidence intervals. All regressions include firm age fixed effects. Standard errors are clustered at the firm-level. The outcomes considered are Log(Sales); Log(Wage bill); Log(Capital); and Log(Debt).

L Theoretical framework

In the current appendix, we develop a simple competitive model of the longer-run effects on firm size of a transient demand shock. The main purpose of this appendix is to show that the five mechanisms discussed in the main text — learning about oneself, sunk costs, learning by doing, financial constraints, and reduction of demand frictions — may all be consistent with longer-run effects on firm size of transient demand shocks.

L.1 Basic setup

A competitive firm has one production factor, capital, and period t production equal to

$$y_t = \theta_t K_t^{\alpha}, t = 1, 2, \dots T. \tag{A.VII}$$

where y_t is the firm's output in period t, θ_t is productivity, K_t is capital employed, and α is a scale parameter less than one. The per-unit cost of capital is r (r is assumed constant over time as it plays no role in the analysis). We assume that capital depreciates at a rate $1 - \delta$, where $0 \leq \delta \leq 1$, so that $K_t = \delta K_{t-1} + J_t$, where J_t is the investment in new capital in period t. We normalize initial capital levels to zero, so that in period 1, $K_1 = J_1$. We further assume that $J_t \geq 0$, i.e., selling capital is not feasible (in other words, investments are sunk). The firm's profits in period t can be expressed as,

$$\pi_t = p_t y_t - rJ_t = p_t \theta_t K_t^{\alpha} - rJ_t \tag{A.VIII}$$

where p_t is the product price in period t. The objective of the firm is to choose $\{J_t\}_{t=1,..T}$ so that discounted profits are maximized. Assuming a zero discount rate for simplicity, the firm maximizes $\sum_t \pi_t$, denoted by Π, with respect to $\{J_t\}_{t=1,..T}$, subject to the non-negativity constraint $J_t \geq 0$ for all t. For convenience we assume throughout that $T = 2$. As way of interpretation, period 1 can be thought of as the period when the transient demand shock occurs — the procurement contract period — and period 2 can be thought of as the post-contract period. A transient demand shock is modeled as $p_1 > p_2$.

In the benchmark analysis, we assume that (i)all parameters are known to the firm (i.e., no learning), (ii)$\delta = 0$, i.e., capital depreciates fully between the two periods (this means that there are no investments), and (iii)the firm faces no financial constraints and can choose whatever $I_t \geq 0$ it sees fit. These three assumptions are modified later.

L.2 Benchmark

We first solve the model under the assumptions (i)-(iii). In the main text, this is referred to as the simple neoclassical setting. As capital depreciates fully after the first period, the profit maximization problem in period 2 is identical to that in period 1. Thus, maximizing Π is equivalent to maximizing each π_t with respect to I_t separately. The first order conditions become,

$$\frac{d\pi_t}{dJ_t} = \frac{d\pi_t}{dK_t} = p_t \frac{dy_t}{dK_t} - r = p_t \alpha \theta_t K_t^{\alpha-1} - r = 0 \tag{A.IX}$$

which gives solution, denoted by K_t^* equal to,

$$K_t^* = J_t^* = (\frac{r}{p_t \alpha \theta_t})^{\frac{1}{\alpha-1}} = (\frac{p_t \alpha \theta_t}{r})^{\frac{1}{1-\alpha}} \tag{A.X}$$

We see directly from (A.X) that K_2^* is independent of p_1. This means that, in the benchmark model, a temporary demand shock in period 1, as measured by an increase in p_1, will have no effect on firm size in period 2, as measured by either K_2 or y_2.

L.3 Learning about oneself

We modify assumption (i) by letting the productivity parameters θ_1 and θ_2 be initially unknown (all the other parameters are still known). For simplicity assume that $\theta_1 = \theta_2$, and suppose that θ_t may take on two values, 0 or 1, the latter with probability θ. Thus $E(\theta_t) = (1-\theta)\cdot 0+\theta\cdot 1 = \theta$. Let us further assume that the firm learns its type between period 1 and period 2 with probability x. As long as x is independent of the firm's choice of K_1, the firm's optimal investment policy in period 1 is independent of its investment policy in period 2, because capital fully depreciates between the two periods. Expected profits in period 1 therefore equal,

$$E(\pi_1) = E(p_1 y_1 - rK_1) = E(\theta_1 K_1^\alpha - rK_1) \tag{A.XI}$$
$$= \theta K_1^\alpha - rK_1$$

which gives optimal capital level in period 1 equal to,

$$K_1^* = J_1^* = (\frac{p_1 \alpha \theta}{r})^{\frac{1}{1-\alpha}} \tag{A.XII}$$

This is the same expression as in (A.VIII) with θ_t replaced by θ. The analysis of period 2 is straightforward. If the firm does not learn its type between period 1 and period 2, which occurs with probability $1 - x$, the profit maximization problem is the same as in period 1, with the same solution. In that case, the firm stays in business and chooses the same capital level as in period 1. If, however, the firm learns its type after period 1, which happens with probability x (the argument goes through also when $x = 1$), its optimal choice in period 2 is either to exit (if $\theta_t = 0$) or to choose $K_2^* = (\frac{p_2 \alpha}{r})^{\frac{1}{1-\alpha}}$ (if $\theta_t = 1$).[32] The latter expression follows directly from equation (A.X).[33] Thus with learning, the firm will exit with probability $x(1-\theta)$ between the

[32] A small fixed cost incurred each period would make it strictly optimal for the firm to exit. We abstract from such fixed costs to minimize notation.

[33] If $p_1 = p_2$ then it follows directly from equation (A.X) that $K_1^* < K_2^*$, i.e., the firm expands from period 1 to period 2.

two periods.

Let us suppose that auction winners learn more about their own productivity than auction losers in the contract period, and for simplicity assume that losers learn nothing and winners learn their type with probability x. It follows from the derivation above that auction winners will be more likely than auction losers to close down. Those that lose the auction (and do not learn about type) will choose the same level of capital in period 1 and in period 2, and hence stay in operation, while auction winners will close down with probability $x(1-\theta)$.

L.4 Sunk costs

We now consider the effect of sunk costs. We revert to the basic setup but modify assumption (ii) by letting $\delta > 0$, i.e., capital does not depreciate fully between the periods. Assumptions (i) and (iii) are retained. For simplicity, let $\theta_1 = \theta_2 = \theta$. We solve the problem backwards, starting with period 2. Period 2 profits equal,

$$\pi_2 = p_2 y_2 - rI_2 = p_2\theta K_2^{\alpha} - rJ_2 \tag{A.XIII}$$

Note that the capital base transferred from period 1 to period 2 is $\delta K_1 > 0$, so that period 2 capital level is equal to $\delta K_1 + J_2$. Treating K_1 as given and substituting the accounting identity $J_2 = K_2 - \delta K_1$ into (A.XIII),

$$\pi_2 = p_2 y_2 - rI_2 = p_2\theta K_2^{\alpha} - r(K_2 - \delta K_1) \tag{A.XIV}$$

By the same derivation as before, the solution for K_2^* becomes,

$$K_2^* = (\frac{p_2\alpha\theta}{r})^{\frac{1}{1-\alpha}} \tag{A.XV}$$

The expression in (A.XV) is identical to the one given by equation (A.X). There are two cases to consider. The first is when $\delta K_1 \geq K_2^*$. In that case, $J_2^* = 0$. The economic interpretation of this case is that capital is abundant in period 2 due to large investments being made in period 1, so the constraint $J_2 \geq 0$ is binding. The second case is when $\delta K_1 < K_2^*$. Then we have that $J_2^* = K_2^* - \delta K_1 > 0$. In other words, the firm invests what is sufficient to raise the capital level to K_2^* as this is the investment level that raises capital level in period 2 to the optimal level.

We are now interested in the effect of a temporary demand shock, i.e., a high p_1, on firm size in period 2. It is easy to show, and hence omitted, that K_1^* will be a strictly increasing function of p_1. Moreover, K_2^* does not depend directly on p_1 as shown in (A.XV). Therefore, there must exist k such that $\delta K_1^* < K_2^*$ for $p_1 < k$ and $\delta K_1^* \geq K_2^*$ for $p_1 \geq k$. For $p_1 < k$, firm size in period 2 is independent of p_1, because the firm in period 2 sets its investment level so that K_2^* is reached regardless (recall that $\delta K_1^* < K_2^*$ in this case). For $p_1 > k$, firm size in period 2 increases in p_1, because the higher p_1 the more capital is left over from period 1.

Thus if the firm faces a temporary demand shock, it will build up a large capital base in period 1, and K_2^* will be large via the "hangover" channel δK_1 (even if no investments are made in period 2). Therefore firm size in period 2 will be larger if the firm experiences a temporary demand shock in period 1.

L.5 Learning by doing

Let us modify the model by introducing learning by doing. We retain assumptions (i)-(iii) but assume that productivity of the firm increases over time, and more so the higher activity in the previous period,

$$\theta_2 = \theta_1 + \Delta(y_1) \tag{A.XVI}$$

where $\Delta(y_1)$ is some strictly increasing function with $\Delta(0) = 0$ and $\Delta(\infty)$ bounded. Note that the learning-by-doing mechanism makes investments in period 1 more valuable than in period

2, due to the spillover effect. We solve the problem backwards, starting with period 2. In period 2, profits equal,

$$\pi_2 = p_2 y_2 - r I_2 = p_2 \theta_2 K_2^{\alpha} - r J_2 \tag{A.XVII}$$

Note that θ_2 is fixed in period 2. By the same derivation as before, the solution for K_2^* becomes,

$$K_2^* = (\frac{p_2 \alpha \theta_2}{r})^{\frac{1}{1-\alpha}} \tag{A.XVIII}$$

We are now interested in the effect of a temporary demand shock, i.e., a high p_1, on firm size in period 2. First note that K_2^* does not depend directly on p_1. It is easy to show, and hence omitted, that K_1^* will be a strictly increasing function of p_1. Therefore y_1 will be strictly increasing in p_1, and so will θ_2 from (A.XVI). It follows from (A.XVIII) that K_2^* is a strictly increasing function of p_1. Thus a temporary demand shock will increase production in period 1, make the firm more productive in period 2, and via investments made in period 2 lead to the firm being larger in period 2.

L.6 Financial constraints

Let us now modify the model to accommodate financial constraints. We retain the assumption $\delta = 0$, which now means that Π is separable. Moreover we assume for simplicity that $p_1 = p_2 = p$ and $\theta_1 = \theta_2 = \theta$. In that case, the optimal investment level is the same in both periods,

$$J_1^* = J_2^* = (\frac{p \alpha \theta}{r})^{\frac{1}{1-\alpha}} \tag{A.XIX}$$

As in Evans & Jovanovic (1989), we model financial constraints as the firm having available only W_1 for investment in period 1. The interesting case is $W_1 < r J_1^*$, i.e., the firm is financially constrained in period 1 and must operate at below first best firm size. We denote the feasible investment level in period 2 for W_2 In period 2, the firm has available the revenues generated from the first period, i.e., $p_1 y_1$ (we abstract from the revenue generating a positive interest payment). But as the firm is generating a positive profit in the first period, $p_1 y_1 > W_1$. It follows that $W_2 > W_1$. Therefore the firm's investment level is higher in period 2 than in period 1, which is also true for capital level and sales.

L.7 Demand frictions

To illustrate the effects of transient demand shocks on firm size under demand frictions, we retain assumptions (i)-(iii) but assume that demand in period 2 depends on firm sales in period 1, in the following way,

$$p_2 = p_1 + \psi(y_1) \tag{A.XX}$$

where $\psi(y_1)$ is some strictly increasing function with $\psi(0) = 0$ and $\psi(\infty)$ bounded. Thus, the firm faces higher demand in period 2 the higher are sales in period 1. Note that the demand friction mechanism makes investments in period 1 more valuable than in period 2, due to the spillover effect. We assume for simplicity that $\theta_1 = \theta_2 = \theta$, and solve the problem backwards, starting with period 2. In period 2, profits equal,

$$\pi_2 = p_2 y_2 - r I_2 = p_2 \theta K_2^{\alpha} - r J_2 \tag{A.XXI}$$

By the same derivation as before, the solution for K_2^* becomes,

$$K_2^* = (\frac{p_2 \alpha \theta}{r})^{\frac{1}{1-\alpha}} \tag{A.XXII}$$

We now analyze the effect of a temporary demand shock, i.e., a high p_1, on firm size in period 2. First note that K_2^* does not depend directly on p_1. It is easy to show, and hence omitted, that K_1^* will be a strictly increasing function of p_1. Therefore y_1 will be strictly increasing in p_1, and so will p_2 from (A.XX). It follows from (A.XXII) that K_2^* is a strictly increasing function of p_1.

Thus, a temporary demand shock will increase sales in period 1, make demand higher in period 2, and via investments made in period 2 lead to the firm being larger in period 2.

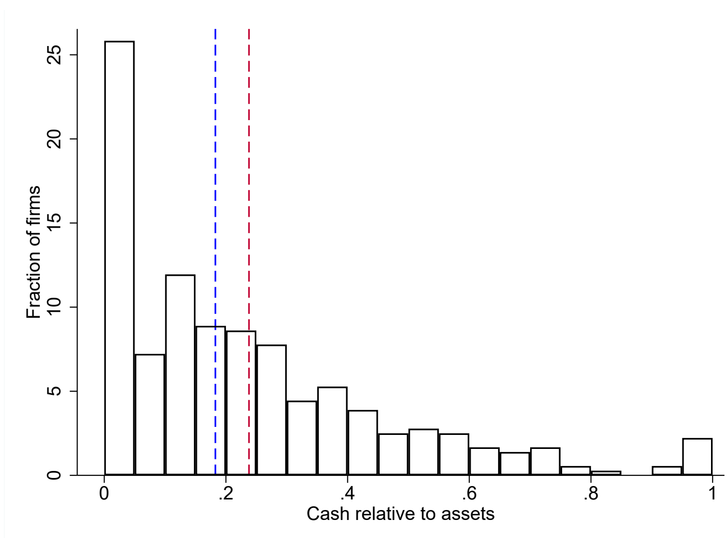
L.8 Summary

The current appendix has shown in that in a simple competitive setting, a transient demand shock can produce lasting effects on firm size through all the mechanisms described in the main text — learning about oneself, sunk costs, learning by doing, financial constraints, and reduced demand frictions. In this appendix, we modeled the transient demand shock as a temporarily high product price. Alternatively, one could model the demand shock as a constant product price but a temporary increase in volume, i.e., one where the competitive firm is rationed. We expect the same conclusion to emerge from such an exercise, and for the same reasons.

M Effects for cash rich and cash poor firms

In Section 5, we estimate the effects of procurement wins on startup outcomes using a difference-in-differences design. In the current appendix, we re-estimate our main results for Log(Sales) and Log(Wages) for samples of cash rich and cash poor firms. Firms are cash rich if their cash-to-asset ratio exceeds 0.2 (roughly corresponding to the mean and median in Figure A.VIII). Firms are cash poor if their cash-to-assets ratio is lower than 0.2. The difference-in-differences results for samples of cash rich and poor firms are presented in Table A.XI. A small number of firms cannot be classified as neither rich nor poor because we do not observe the firm's cash holdings. For this reason, the number of observations do not sum across columns in Table A.XI.

Figure A.VIII: Distribution of cash-to-assets (in $t = -1$)



Note: The figure presents the distribution of cash-to-assets for our startup sample in the year before the treatment auction. The blue dotted line represents the sample median. The red dotted line represents the sample mean.

Table A.XI: Effects of auction wins for cash rich and cash poor firms

	Log(Sales)			Log(Wages)		
	Baseline	Cash rich	Cash poor	Baseline	Cash rich	Cash poor
θ (Contract)	0.25***	0.29**	0.20	0.14*	0.13	0.15
	(2.81)	(2.59)	(1.44)	(1.88)	(1.29)	(1.32)
θ (Post-contract)	0.24**	0.28**	0.21	0.23***	0.32***	0.16
	(2.34)	(2.24)	(1.31)	(2.91)	(3.06)	(1.28)
Adj R^2	0.76	0.73	0.78	0.81	0.81	0.82
N	2945	1572	1351	2843	1533	1288

Note: The table reports estimates of θ from the following regression: $y_{jek} = \theta b_{jek} + \kappa_t + \alpha_e + \lambda_{jk} + \varepsilon_{jek}$, where y_{jek} is the outcome for firm j in event-time e centered on auction k; b_{jek} equals one for auction winners in the post-auction period, zero otherwise; and κ_t, α_e, and λ_{jk} are calendar-time, event-time, and firm-in-auction fixed effects, respectively. The coefficient θ is allowed to differ in the procurement contract-period and post-contract period, and is estimated separately for the full sample of firms (Baseline), cash rich firms (Cash rich), and cash poor (Cash poor) firms. The outcomes are Log(Sales) and Log(Wage bill). Standard errors are clustered at the firm-level. t-statistics in parentheses. Stars (***, **, *) indicate statistical significance at the 10, 5, and 1 percent levels.

Table A.XII: Comparing cash rich and cash poor firms

	Firm characteristics				
	Sales	Employees	Total assets	Profits	Winnings/Sales
Rich	-9434.01**	-10.06**	-8405.12*	559.42	0.16
	(-2.29)	(-2.16)	(-1.74)	(1.63)	(0.76)
Adj R^2	0.01	0.01	0.00	0.00	-0.00
N	325	325	325	325	181

Note: The table reports estimates of the difference-in-means in firm characteristics between cash rich and cash poor firms in the year before the focal auction. The firm characteristics are sales, number of employees, total assets, profits, and auction winnings divided by sales (only defined for auction winners). t-statistics in parentheses. Stars (***, **, *) indicate statistical significance at the 10, 5, and 1 percent levels.

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